Vol. 36, No. 4 April, 1939

Psychological Bulletin

PSYCHOLOGY AND THE SCIENCE OF SCIENCE

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The scientist has always been proud of his hard head and his tough mind. When William James (29) sat in judgment and divided the universe of temperaments into the tough- and tender-minded, the scientist knew where he belonged. He was happy to run with the goats, for he was an empiricist and he loved facts in all their crude variety. He was skeptical and cautious of word, and to 'isms' of all kinds he was peculiarly unresponsive. The tender-minded were the rationalists. They had faith in intuition and were awed by the power of the mind. It was their opinion that by taking thought they could discover absolute principles of truth answering to the criteria of coherence and consistency and that, armed with these principles, they could legislate the bounds of science. They were the sheep whose wool shone white under the light of reason. They were most numerous in departments of philosophy.

Undoubtedly these two types are still with us, but it is the purpose of this review neither to shear the sheep nor tame the goats. Instead, its purpose is simply to invite attention to some recent developments in what we might call the Philosophy of Science.

The tough-minded scientist has always known that he could screen his integrity against the seductive pipings of the rationalist by ignoring philosophy. The tender-minded philosopher, gifted with his superior dialectic, has usually despaired at the stubborn naïveté of the scientist and has determined to leave the unrefined fellow to grovel alone, while he, the philosopher, calmly demonstrated the impossibility of proving anything by induction. Suddenly, however, we find, on the one hand, a coterie of philosophers plying us with what, if it is not science, is certainly not the brand of stuff we have ordinarily pigeonholed as philosophy; and, on the other hand, we

are beset by a host of scientists of all disciplines campaigning for what, if it is not philosophy, is surely not the science we are used to.

The philosopher, Benjamin (3), says of these scientists:

"They begin with science, they talk about science, and they end with science, yet they do not conform at all to the tradition of scientific writing. . . Their repeated reference to philosophical issues tempts one to classify them with this group, yet the writings approach these problems in a new spirit and with a new method, which seem quite foreign to traditional philosophy."

And concerning the widespread groups of philosophers participating in this movement, Reichenbach (51) observes:

"Though there is no philosophic system which unites these groups, there is a common property of ideas, principles, criticisms, and working methods. . . It is the intention of uniting both the empiricist conception of modern science and the formalistic conception of logic . . . which marks the working program of this philosophic movement."

So numerous and insistent are the words of those who have been seized by the spirit of this movement that they swell the pages of several new journals—journals whose subject matter defies simple classification.[1] There are articles by philosophers, mathematicians, and scientists. But it is more than a mere scrambling of the sheep and the goats. A common spirit animates most of these writings. The common theme, despite its fundamental simplicity, despite differences of interpretation by newborn enthusiasts, and despite the disparagement of misunderstanding, is probably to be esteemed as a truly great advance in the Philosophy of Science, or the Science of Philosophy.

Numerous phrasings of this central theme have been cast by authors interested in various aspects of it, but they all assert essentially that *science seeks to generate confirmable propositions by fitting a formal system of symbols (language, mathematics, logic) to empirical observations, and that the propositions of science have empirical significance only when their truth can be demonstrated by a set of concrete operations.* There are thus two separate realms of discourse:

[1] Some representative journals are: *Erkenntnis,* begun in 1930; *Philosophy of Science,* begun in 1934; and *International Encyclopedia of Unified Science,* begun in 1938. The advisory boards of these last two publications read like a who's who in science and philosophy. It would be a passionate optimist, however, who would expect such a band of hardy individualists to be entirely of one mind. Many of them have not yet spoken.

the *formal* (or rational) and the *empirical*. It is the business of the philosopher to labor with the formal and discover and perfect the rules of the scientific language, and it is the business of the scientist to apply the formal symbolic model to the observable world in such a way that the concepts he generates will satisfy the rules of operational criticism.

Elementary as these notions may appear, the development of their implications has commanded the interest of both tough- and tender-minded. The movement has proved disastrous for metaphysics, challenging for logic, and salutary for science. Philosophers and scientists in essential agreement are astonishing enough, but here we have them pleading for a common method. In this strange harmony we are witnessing the birth of a new discipline: the Science of Science. It is a triumph for self-consciousness. The science-makers are asking themselves how they make science and are turning on that problem the powerful empirical weapons of science itself; while at the same time a tough-minded outcropping among the philosophers is carefully combing the metaphysics out of logic in order to investigate more easily the common linguistic structure of science. In this quest the philosophers, like the scientists, resort to empirical methods. Witness the spirit of philosophy as exemplified by Nagel (**43**):

> "It is difficult for me to take seriously the various attempts made historically by philosophers to legislate to the sciences just what they can and cannot investigate . . . on the basis of a deductive theory of mind and nature. . . Furthermore, it seems to me an integral character of skilled workmanship to insist upon the fact that no statement or proposal has any meaning apart from the methods which are or may be employed to establish or execute them."

In succeeding pages we shall see how operationism, beginning at one end in the laboratories of scientists, evolved an enterprise coördinate with that of Logical Positivism, Physicalism, and Scientific Empiricism which, beginning at the other end in the armchairs of philosophers, settled on the problem of the proper scientific use of logic. And we shall see how the natural issue of this mating came to make up the unifying principles of the Science of Science. We shall see how this movement concords with 'behavioristics,' which is a behavioristic psychology tuned up to keep pace with a fast-moving logical criticism. And finally, we shall see what the impact of this movement means for some specific problems in psychology, and what is indicated as the future rôle of psychology in this scheme.

Operationism

Ten years ago Professor Bridgman, the expert on high-pressure phenomena, wrote a book [2] called *The logic of modern physics* (11). It has been judged an excellent book, animated by the single idea that "in general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*" This dictum stands forth in what many have found to be objectionable nakedness, but, throughout more than 200 well-stocked pages, Bridgman demonstrates what he means by analyzing the operational meaning of the basic concepts of physics. There is nothing rationally *a priori* in his method (at least he honestly *tries* to exclude metaphysics). His introductory confession is: "The material of this essay is largely obtained by observation of the actual currents of opinion in physics." In this empirical spirit he observes the behavior of his colleagues and finds that what is considered an *explanation* "consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests." The reduction of the "situation" is made in terms of operations, but do we thereby arrive at exact and certain knowledge? No. "We never have perfectly clean-cut knowledge of anything, but all our experience is surrounded by a twilight zone, a penumbra of uncertainty, into which we have not yet penetrated," and consequently "no empirical science can ever make exact statements." The degree to which any of the laws of science wear the penumbrous halo can be told only by inspecting the operations which the laws are intended to generalize.

Bridgman's book is rich in example but poor in precept. That its author has occasionally been misunderstood has perhaps been due largely to this fact. The book gives numerous examples of operational method without prescribing explicitly what operational method is; it talks of "operations" without giving an explicit definition of the term; and it discourses on natural laws without pointing out how we get from particular operations to generalizations. In short, it is a thoroughly inductive enterprise, and the reader is often allowed to make the induction by himself. Nevertheless, the spirit of the book is unmistakable and its message is simple and powerful.

Philosophers rose to protest, or sometimes to defend, the notion of "operational meaning" because it assures the automatic elimination of even the choicest propositions of metaphysics: "If a specific

[2] For additional comments on some of the books and papers cited in this review see the "Bibliography."

question has meaning," says Bridgman, "it must be possible to find operations by which an answer may be given to it." No operations, no meaning! And so, as we have said, philosophers, and others, rose to protest. Finally, the pressure pushed Bridgman temporarily from his Harvard laboratory and on to the lecture platform at Princeton where he spoke what became another book: *The nature of physical theory* (13).

To say that this second book pleased all who were disciples of the first book is perhaps not quite true. The author had been able to say in his first book that fortunately he would "be able to get along with a more or less naïve attitude toward" psychology and epistemology, but in his second work he boldly lays hold on thought, language, and experience. Bridgman's discussion of these concepts was what the world had been waiting for, but once out of the well-charted sea of physics and adrift in epistemology, the author's bark, if we are to believe the critics, appears to have lost its rudder. One cannot avoid the impression that criticism of this second book has been of unmerited severity, but perhaps severe criticism is what must be expected by a man who challenges us with issues as vital as those proposed by Professor Bridgman. Objection has been made to such statements as: "In the last analysis science is only my private science." "What," asks editor Malisoff (34), "can an operationist mean by a 'last analysis'?" Bridgman says his purpose in sailing the epistemological waters is "to map out the possibilities and limitations of the human mind in dealing with the problems presented to it." "Our complaint," criticizes A. F. Bentley (6), "is not that he makes this inquiry, but that in it he employs all the bad devices he had ejected from physics." Some of his devices are assertions crutched on such terms as "essentially," "absolutely," and "intuitively." Nevertheless, Bridgman's critics agree that his discernment in physics remains as fine as ever—he is still simple and hardheaded. In physics he is an operationist, and it is in physics that we should judge him, if we are to presume to do so.

Just as Bridgman had set out to apply and make explicit the principles by which Einstein shattered the physicist's notion of the absolute, so did others seize upon the opportunity to try out these principles in other fields. Psychologists, long self-conscious of their own self-consciousness, were particularly alert to this budding self-inspection on the part of the modern masters of physics. If the physicists could examine the methods of their science-making and evolve helpful principles, perhaps psychologists could do likewise.

Such, at least, was the attitude of those who were happy to confess blindness to any fundamental dichotomy between the methods of psychology and physics.

Operationism in Psychology

But psychology is more difficult than physics—at least psychologists often find it easier to get themselves into a mess in their field than physicists do in theirs. Of course, when the physicist strays into psychology, the result is apt to restore the psychologist's ego-level, but if the physicist fumbles it only serves to show that when doing open-field running among psychological concepts the critic must hold the ball more tightly. In view of the difficulty of keeping a grip on the operational principle, it is not surprising to find evidence of dissension among the psychological apologists (Professor Bills (7) calls them "apostles"). In spite of much scattered writing, the case for operationism in psychology has perhaps never been adequately briefed, but a few of its consequences have been made explicit, and some interesting applications have appeared.

We all remember Tolman's *Purposive behavior in animals and men* (60). Whatever dismay we may have felt at the superabundance of his glossary, the fact remains that in coining the words of his new language he appealed most directly and explicitly to experimental operations. The book is a monument in the methodology of definition. In much the same spirit, Professor Tolman has more recently prepared for us "an operational analysis of 'demands'" (61). In his own field of expertness we find McGeoch making a critical inquiry into the possibility of "learning as an operationally defined concept" (37). Boring treats of "temporal perception and operationism" (10) in a short, poignant demonstration of how a classical problem turns out to be specious when its defining operations are made explicit. Seashore and Katz propose to bring order to the chaotic discipline of abnormal psychology by "an operational definition and classification of mental mechanisms" (54). Lundberg, the sociologist, would do the same for the social sciences by replacing spineless intuitionism by "quantitative methods in social psychology" (33), the foundation for which would be concepts operationally defined. And finally, Kantor examines "the operational principle in the physical and psychological sciences" (30) and concludes that the principle, properly enlarged, can be employed to the psychologists' advantage. Now, these are

not all of those who have taken notice of Bridgman's proposals. Nor do all these commentators see eye to eye with Bridgman or with each other regarding certain fundamentals. Furthermore, it is becoming alarmingly obvious that the phrases 'operationally defined' and 'operationally sound' are acquiring the sort of positive valence which leads to their being bandied about in indiscriminate fashion by writers who suppose that they can meet the operational test by announcing good intentions. Operationism is being threatened by its friends—largely, perhaps, because of the inherent difficulty of making a rigorous formulation of it.

What, then, are we to understand by operationism? All that any one man can do is to present his own version, and this I did in a series of articles in 1935 and 1936 (**56**, **57**, **58**). There are some points there which invite revision, but, in general, the sins appear to be those of omission. The statement there needs expansion, but obviously this review is not the place for it. A résumé is more in order.

First, however, it must be emphasized again that the development of operational principles is properly an empirical undertaking. What do the science-makers do? What methodology has the maximum survival value? When do propositions have empirical validity? In short, operational principles are induced generalizations rather than *a priori* fiats. They are therefore subject to the usual hazards and uncertainty of inductive propositions. This empirical aspect of operational criticism has never been sufficiently stressed, and it is not surprising that operationists have sometimes been regarded as self-appointed legislators who try to prescribe rather than discover.

These, then, are some of the generalizations which I propose as verifiable:

1. Science, as we find it, is a set of empirical propositions agreed upon by members of society. This agreement may be always in a state of flux, but persistent disagreement leads eventually to rejection. Bridgman does not agree to this social criterion of knowledge and it was against this notion that he aimed a part of his Princeton lectures (**13**). We must ask him, however, to produce the negative case. A physical law to which only Bridgman agreed would not be a part of physics—not, at least, until he won converts, and then there would be agreement.

2. Only those propositions based upon operations which are public and repeatable are admitted to the body of science. Not even

psychology knows anything about private experience, because an operation for penetrating privacy is self-contradictory.

3. What becomes acceptable psychology accrues only when all observations, including those which a psychologist makes upon himself, are treated as though made upon 'the other one.' Thus, we make explicit the distinction between the experimenter and the thing observed. This distinction is obvious in physics; in psychology it is equally valid.

4. Although a particular experimenter may himself become the object of study by another experimenter, and he in turn by still another, at some stage of such a regress an independent experimenter *must be* (*i.e.* is always) assumed. The recognition of this 'experimenter-regress' unravels many knots in psychology.

5. A term denotes something only when there are concrete criteria for its applicability; and a proposition has empirical meaning only when the criteria of its truth or falsity consist of concrete operations which can be performed upon demand.

6. When we attempt to reduce complex operations to simpler and simpler ones, we find in the end that discrimination, or differential response, is the fundamental operation. Discrimination is prerequisite even to the operation of denoting or 'pointing to,' because whenever two people reduce their complex operations for the purpose of reaching agreement or understanding, they find that unless they can each discriminate the same simple objects or read the same scales they still will not agree. Agreement is usually reached in practice before these most elementary operations are appealed to.

7. There are two types of propositions: *formal* and *empirical*. The formal propositions are arrays of symbols without empirical reference. They are language, mathematics, and logic *as such*. Empirical propositions are those in which these arrays of symbols have been identified with observable events. Sometimes the two types of propositions intermingle and trouble results. For avoiding the obscurity of pseudo problems this distinction between the formal, syntactical model (symbols) and the operations for which it is made to stand is of prime importance. Hypotheses, for example, can be only formal statements—operationally empty—until they are demonstrated (see Appendix II). Within the formal realm we speak sometimes of mathematical operations, but here we mean the manipulation of

symbols carried out according to certain conventional rules. These are not the operations of operationism.

Although we shall have more to say later about the contrast between the formal and the empirical, at this point we might do well to see how history occasionally sets them off from one another and thereby emphasizes their distinctive natures. Historically, the algebra of complex numbers (numbers of the form $x+iy$, where x and y are real numbers and i is the square root of -1) was developed from the purest of purely mathematical motives. The rules for the manipulation of these numbers (their addition, multiplication, division, etc.) were worked out in conformity with the conventional laws of ordinary algebra and interesting relations were discovered. Gauss, for example, set a landmark in algebra by proving that every algebraic equation in 1 unknown has a root and that all roots of such equations are complex numbers (see Bell, 2, p. 232). In Gauss's time these numbers were simply abstract symbols which could be combined according to the rules of the game we call algebra. They proved nothing about the empirical world or about science: they constituted, as they still do, a purely *formal* system. Then, with the advent of alternating electric currents, came also the need for a simple, effective 'model' to represent electric circuits; and the electrical engineers discovered that if they let x stand for resistance, iy for inductive reactance, and $-iy$ for capacitative reactance, they could manipulate these symbols according to the rules of complex algebra and obtain new combinations of the symbols which they could then identify with some measurable aspect of an electric circuit. In other words, this formal system was found useful as a model, and out of its utility has grown the modern intricate theory of alternating currents. Therefore, when we can identify these complex numbers with various aspects of a circuit, we can say that the propositions containing these symbols are *empirical* propositions, testable by concrete operations.

These seven bald assertions about operationism are perhaps too brief to be convincing, but they may recommend the fuller development in the three papers already referred to. In the meantime we might profit by considering what operationism is not—still, of course, in only one man's opinion. Misunderstandings have been numerous and many of them could have been headed off had someone signaled what is nonoperational. Let us, then, look at a few of operationism's contrasts.

What Operationism Is Not

1. It is obviously not a new school of psychology. Rosenzweig (52) presented an admirable argument to show that the schools of psychology are really more complementary than antagonistic, but he was worried about operationism. He should stop worrying.

2. It is not a set of rules telling how to be a bright and original scientist. It does not even tell how experiments should be carried out. It has no precepts. At the risk of breeding disappointment we must say of operationism, as James (29) said of pragmatism, that, "at the outset, at least, it stands for no particular results. It has no dogmas, and no doctrines save its method." Furthermore, its method is one which is applied *after* the scientific proposition has been made: it provides criteria for determining whether what *has been* said is empirically meaningful. In short, it tests inventions, but does not tell how to invent.

3. It is not opposed to hypotheses, theories, or speculation. It seeks merely to discover criteria by which these things may be detected and labeled. It is not opposed to poetry, art, or religion. It wants only to know the difference between these things and science. It wants to know under what conditions the consorting of science with metaphysics breeds pseudo problems. Scientists as people may be opposed to pseudo problems, but operationism's business, as a principle of criticism, is to discover them.

4. It is not a guarantee of agreement as to tastes or theories, but it points out how agreement as to facts is achieved by men capable of making the same fundamental discriminations. Operationism wants, most of all, to discover the bases of such agreement. What are the procedures which compel agreement among those engaged in open-minded pursuit of science? As to compelling agreement on tastes—that is probably a job in applied eugenics.

5. It is not positivism. The blemish on positivism was that in its reaction against rational metaphysics it pretended to base *everything* in science on experience. Operationism, however, acknowledges the rôle of the rational methods of mathematics and logic—formal disciplines which do not appeal to experience for verification, but only to conventions. Science uses these formal systems as models for representing its data. To deny them is to cure the disease by burying the patient.

When it is a matter of the significance of *empirical* rather than of *formal* propositions, needless to say, operationism adopts an uncompromising positivistic attitude.

6. It is not behaviorism. Like positivism, behaviorism erred in denying too much. Operationism does not deny images, for example, but asks: What is the operational definition of the term 'image'? Of course there are different behaviorisms, and some of the renovated brands are truly operational. Tolman (62) has a variety which he dubs explicitly "operational behaviorism"—and perhaps it is. It is certain that the behavioristic emphasis has served capably in blasting a path through subjectivity, and without this path an objective Science of Science could not march.

7. It is not monism. It asks only whether any operational meaning can be given the proposition that there is but one irreducible substance or attribute. Can the truth or falsity of this proposition be put to experimental test? If not, we face a pseudo problem.

8. It is not dualism. Here again the problems raised are pseudo problems, because the propositions are not testable. As Bills (7) so aptly says, "Parallelism would automatically reduce to a double-aspect formula, because where two sets of defining operations coincide perfectly they become identical operationally." Of course there can be no quarrel, except on the grounds of utility, with any arbitrary dividing or classifying of facts, but pseudo problems can be avoided only provided we remember that these classes are arbitrary.

The division of concepts into the categories of subjective and objective is justifiable—if at all—only on pragmatic grounds, and only *provided* both types of concept answer the operational test. Bills believes that "mentalistic" concepts like percept, image, and idea can be operationally defined. So do I. Kantor, however, is disturbed. He detects dualism. But Bills "cannot agree with Kantor that there is any necessary dualism implied in Stevens' position." Neither can Stevens. If we admit to our store of empirical science only those concepts which are operationally founded, can we not classify them according to our purposes?

Kantor (30) would appear to supplant dualism with a kind of realism. Now, realism is a metaphysical doctrine, and perhaps Kantor did not intend a realism. Nevertheless, he appears to defend the proposition: Nature is not the same as our knowledge of nature. Operationism must here again pose its perhaps tiresome, but necessary, question: Can any operations be formulated which will either

prove or disprove this proposition? If not, it is operationally meaningless, however much 'emotional meaning' it may pack.

9. Finally, operationism is not pluralism. It should be apparent by now that operationism is not consonant with any 'ism' which asserts something about the ultimate nature of reality.

The Problem of Generality

There is one more criticism we must take seriously before we continue. It has been urged that operationism reduces to a vicious particularism; that there is no provision for generalization; that instead of unification in science a strict servility to the operational principle nourishes an ever-expanding multiplicity of concepts. Here is what the critics say:

Margenau, in "Causality in modern physics" (35), which he addressed to the philosophers, states that operationism "cannot be tolerated as a general directive. For, in the first place, it would, if carried to its consequences, dissolve the world into an unmanageable variety of discrete concepts without logical coherence."

Lindsay, in "A critique of operationalism in physics" (31), says: ". . . logically the operational method . . . implies that each concept is tied to a definite operation."

Lindsay and Margenau together, in their book, *Foundations of physics* (32)—a book which has brought them merited high praise, state: "On the basis of purely operational definitions, all concepts are strictly empirical and isolated" (p. 412).

Bills, in his excellent address on "Changing views of psychology as science" (7), says: "One of the ideals of scientific concept-makers is to reduce all concepts to a few fundamental ones. . . Yet this is not, by any means, the likely outcome of operationally defined concepts. . . For there is no universal set of operations."

Waters and Pennington, in their careful criticism of "Operationism in psychology" (63), assert: "The fact that the concept, for Bridgman, is *synonymous* with a corresponding set of operations cannot be overemphasized. The slightest change in any aspect of a set of operations would mean, therefore, a new concept and would demand, likewise, a new symbol for its designation. A multiplicity of concepts could scarcely be avoided."

Since Bentley (the critic, not the psychologist), in his flashy tirade on "Physicists and fairies" (6), has a point to make here, we will let him speak first. He refers to Lindsay and Margenau when

he says: "By distorting Bridgman grossly enough, either man can, of course, readily destroy what he has distorted. Both men distort alike; first by insisting 'operations' must be all hands and no mind; second by alleging that no operation in this world can have anything to do with any other operation, not even with its own repetitions of itself."

Whether there is distortion or not, the fact that so many have pounced on this supposed snare in operationism means that the rules and procedure for generalizing from operations must sometime be made explicit. These rules obviously can be stated, because science does generalize, and operationism seeks only to discover how scientists do what they do.

The process of generalization proceeds on the basis of the notion of classes. All objects or events satisfying certain criteria we call members of a class and to that class we assign a name or symbol. Common nouns originate in precisely this fashion, and it is apparent at once that no empirical proposition is ever without some element of generality. Classification can proceed only when we have criteria defining the conditions for class-inclusion, and these criteria are essentially operational tests. Thus the statement, "Dobbin is a horse," asserts that Dobbin is a member of a class. This proposition is empirically meaningful only provided its truth or falsity can be demonstrated by concrete procedures. Does Dobbin satisfy the criteria of the class, *horse?* If he is a certain size and shape, is covered with hair, feeds on oats and hay, etc., we are happy to acknowledge him as a full-fledged horse. But how do we know he meets our tests? Here we resort to that fundamental operation we have already called discrimination. If we can discriminate crucial differences between Dobbin and other animals we have named horses, we reject Dobbin as something not horse. In other words, we 'correlate' our discriminations—those made on Dobbin with those made on other objects—and the 'goodness' of the correlation determines where we shall classify the beast.

It may be objected that we can always tell Dobbin from other horses, *i.e.* discriminate differences, but we still would resent the suggestion that he is not a horse. The answer is that a certain latitude is always allowed—we seldom resort to j.n.d.'s in a case like this—and the amount of the latitude determines the precision of the concept. As Bridgman has insisted, no concept is without its halo of uncertainty, its penumbra. No empirical class is ever watertight; we can always plague the taxonomist with the borderline case.

On the basis of elementary discriminations, then, we make our first rudimentary classes and in doing so we have made the first step toward generalization. From there we advance to form classes of classes [3] and to discover the relations between classes—always, at the empirical level, in keeping with operational criteria. Occasionally we find that from a certain point of view two classes satisfy the same criteria, or are related by a simple law, so that we are enabled to combine them into a more inclusive class under a more generic tag. Nevertheless, in all of these classifications and combinations the same simple rule is followed: We combine operations when they satisfy the criteria of a class; and the concept of that class is defined by the operations which determine inclusion within the class.

The matter can be illustrated by referring again to that example which appears to have been the jumping-off place for the critics: the concept of length. Bridgman's argument is that we measure the length of the table and the distance to a star by two different sets of operations and we have, therefore, two different concepts of length. True enough. And Bridgman proceeds thence to show that when dealing with very large distances or very minute ones, or with distances where velocities are involved, we do well to keep in mind the differences in our defining operations. However, in his concern for the perils of promiscuous class-matings he forgot to tell us when combining is legitimate. Length measured with a rod is different from length measured with a transit, but under certain statable conditions we can muster operations to determine the relation of these two sets of measurements, and, if they meet the proper criteria, we combine them to form a larger class defining length. Of course, if we had no operations for comparing the two lengths, we should have to veto their combination. In short, then, we can and do generalize the concept length, but we do it with operational sanction.

The Philosophical Movement

Just ten years ago, the year Bridgman published his *Logic of modern physics,* there appeared in Vienna a company of scholars bound together by mutual admiration and a common *Weltauffassung*—a scientific philosophy. Their discussions under the leadership

[3] This *empirical* process of forming classes of classes should not be confused with the *logic* of classes, in which the provision for an infinite hierarchy of classes led to the antinomies discovered by Russell. The empirical process has no *necessary* relation to a formal system of logic.

of Professor Schlick accomplished a unitary enthusiasm which came to concrete form in the organization of *Der Wiener Kreis*.[4] The avowed intention of this "Circle" was to replace philosophy by the systematic investigation of the logic of science which, for Carnap, is "nothing other than the logical syntax of the language of science." There are but two kinds of acceptable propositions: *formal* and *empirical*. Formal propositions concern syntax. They state the rules and procedure for combining words or symbols and have no empirical reference. Empirical propositions are assertions about the observable world and their truth or falsity can be tested by means of observational procedures. Since metaphysics consists of statements not susceptible to empirical test, it is either an array of syntactical (formal) sentences or else it is technical nonsense. Mostly it is nonsense. Philosophy must be purged of it; and, once purged, it

[4] Some of the members of the Vienna Circle follow:

Moritz Schlick (1882–1936) fathered the group. Under his professorial paternalism the Circle met, discussed, and found its unity. (Schlick's unfortunate death, at the hand of a crazed student, occurred as he was climbing the steps of the lecture hall.)

Otto Neurath (b. 1882) contributed his own brand of enthusiastic originality. His spirited support of radical new theses provided important inspiration. Neurath coined the designations "Physicalism" and "Unity of Science."

Rudolph Carnap (b. 1891) labored with the problem of syntax—the logical rules of language. His energetic attack on the problem of the actual construction of a fundamental syntax for the "physical" language has created a whole new field of inquiry.

Philipp Frank (b. 1884), a theoretical physicist, applied the new theory of knowledge to the problems of physics.

Hans Hahn (1879–1934), a mathematician, investigated the foundations of mathematics and exact science in the light of the scientific *Weltauffassung* of the Circle.

Friedrich Waismann distinguished himself with an investigation of the logical foundations of mathematical thinking.

In addition to these members of the Vienna Circle there were other groups whose scientific philosophy was so similar as to be scarcely distinguishable. In fact, one of the impressive aspects of this recent philosophical movement is the manner in which a common *Weltauffassung* appeared almost simultaneously among widely scattered groups of scientists, mathematicians, and philosophers. There was the Warsaw Circle, which boasted such able logicians as Tarski (b. 1901) and Lukasiewicz (b. 1878). At Berlin, prior to the recent cultural eclipse, there was another Circle whose outstanding advocate was Reichenbach (b. 1891). Logicians Russell (b. 1872) and Frege (1848–1925) fall into the same tradition, and in America C. W. Morris (b. 1901) is perhaps the best known expositor of the common program. For a more complete listing of names, see Neurath's "Historische Anmerkungen" (44).

becomes the business of philosophy, says the Circle, to investigate the rules of the language we use in formulating our scientific propositions. The goal of such philosophical research is to provide a secure foundation for the sciences.

This movement was not, of course, without its antecedents. Its most immediate point of departure was the famous *Tractatus logico-philosophicus* (65) by Russell's pupil, Ludwig Wittgenstein. The "Tractatus" exhibited the close connection between philosophy and syntax; it made clear the *formal* nature of logic and showed that the rules and proofs of syntax should have no reference to the meaning (empirical designation) of symbols; and it showed that the sentences of metaphysics are pseudo propositions. But the roots of these notions can be traced even back beyond Wittgenstein. All who, like the positivists, struck out at metaphysics; all who, like Kant, sought to conciliate analytic (formal) methods with the synthetic (empirical); and all who, like the British empiricists, assaulted philosophy with logical weapons have something in common with the Vienna Circle. Hume, in particular, except when he was assuming the existence of a transempirical world, caught the spirit. He winds up his "Enquiries concerning human understanding" (28) with this counsel:

> "If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, *Does it contain any abstract reasoning concerning quantity or number* [formal questions]? No. *Does it contain any experimental reasoning concerning matter of fact and existence* [empirical questions]? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion."

A philosophy as distinctive as that of the Vienna Circle must inevitably become an 'ism,' and its disciples, Blumberg and Feigl (8), lost no time in introducing the Circle's program to American scholars under the title of "Logical Positivism." A. F. Bentley (5) promptly raised the question as to whether Logical Positivism is either logical or positive, but in spite of some obvious disadvantages, the name is not entirely unreasonable. Bentley, as his readers know, loves a *bon mot* and has a low threshold for alarm—he is aroused to criticism easily but not unpleasantly. The name Logical Positivism quite properly suggests the union of the formal and the empirical—a union which, in a well-ordered scientific household, is possible and legitimate.

Logical Positivism proposes to tell us how such a household should be run. A certain division of labor is required. The scientist, in his special field, continues to investigate the empirical relations

among the variables he has at hand and these relations he represents by some form of symbolic language. The philosopher complements the scientist by probing the nature and the rules of this symbolic language. Statements about the empirical domain are called object-sentences; statements about language-forms are syntactical sentences. In any special science, such as psychology, both types of sentences frequently occur, because the psychologist must tell us not only about his facts, but also how he intends to use his words and symbols—he must provide his own definitions (see Appendix I). The philosopher, on the other hand, can point out the logical implications of the psychologist's language and help him guard against the vicious combinations of the two types of sentences which lead to pseudo propositions.

Under this program it is not, however, the task of the philosopher to legislate for science. Science can use any logic it finds useful. Carnap (**22**), at this point, proposes a Principle of Tolerance to allay our fears: "It is not our business," he says, "to set up prohibitions, but to arrive at conventions." "*In logic,*" he continues, "*there are no morals.* Everyone is at liberty to build up his own logic, i.e., his own form of language, as he wishes. All that is required of him is that, if he wishes to discuss it, he must state his methods clearly, and give syntactical rules instead of philosophical arguments." Consequently, he who sets out to scrutinize the logic of science must renounce the proud claim that his philosophy sits enthroned above the special sciences. He works in the same field as the specialist, only with a different emphasis. He ponders the logical, formal, syntactical connections. He studies rules which are basically nothing other than conventions and matters of free choice. Hence the labors of the philosopher in that which is his only legitimate domain, the logic of science, are bound to be barren unless they are pursued in close coöperation with the special sciences.

Logical Positivism, then, seeks (1) to clarify the language of science and (2) to investigate the conditions under which empirical propositions are meaningful. The language of science (including syntax, logic, and mathematics) consists of arrays of words or symbols which we assemble according to certain rules. The analytic propositions of syntax and mathematics are absolutely necessary and certain, once the rules of the game have been laid down. These propositions neither tell us anything about the empirical world, nor can they be confuted by experience. They can no more be proved 'true' than can the conventional rules of the game of chess (see

below). They simply record our determination to use words and symbols in a certain fashion.

Mathematics, under this view, is a completely rational and deductive system and nothing is contained in one formula which is not implicit in all formulas. This, to many, is a fearful thought. Poincaré (47) voiced his apprehension by asking: "If all the propositions it enunciates can be deduced one from the other by the rules of formal logic, why is not mathematics reduced to an immense tautology? . . . Shall we admit that the theorems which fill so many volumes are nothing but devious ways of saying that A is A?" The answer appears to be that regardless of how inventive mathematical discoveries may appear to be, they contain nothing not already implicit in the fundamental postulates of the system. The outcome of our symbol-juggling surprises and delights us and fills us with the illusion of discovery, simply because of the limitations of our minds. A man of sufficient intellect would disdain the use of logic and mathematics, for he would see at a glance all that his postulates and definitions implied. He would be aware of all possible discoveries under the rules. The rest of us, however, must continue to do our mathematics stepwise, proceeding from one tautological transformation to the next, and being surprised at the result.

The second aim of Logical Positivism—to discover the conditions of empirical meaning—leads to the notion that an object-sentence is one which is verifiable by means of some concrete procedure. At this point operationism and Logical Positivism are essentially indistinguishable and we shall say no more about them, except to note an error.

This is an error which the Logical Positivists themselves have acknowledged and corrected (cf. Carnap, 21, p. 11), but since the slip was made in what is commonly regarded as psychological territory, we had best have a look at it. The Vienna Circle committed the all too common fallacy: It claimed to find a difference between *knowledge* and *immediate experience* (see Blumberg and Feigl, 8). Knowledge is communicable, but the immediately given is private and noncommunicable. This from the mouth of a Logical Positivist! Indeed, by all the rules they have proposed, this sentence is not a testable proposition, for how shall we demonstrate the existence of the noncommunicable? But, as already indicated, the Logical Positivists have not been stubborn about insisting that it makes sense to talk of the private content of immediate experience as being different from the discriminable and reportable relations between experi-

ences. Their past lapse in this regard is interesting only because it shows how easy it is for even the well-intentioned to talk nonsense when they invade this field of psychology. In "The operational definition of psychological concepts" (57) I have tried to demonstrate that an empirical (operational) definition of immediate experience is possible provided we note precisely what its advocates do when we ask them to indicate an example of it. Almost invariably they point to a situation involving an elementary discrimination such as: "I see red." Elementary discriminations, then, are what is meant by the immediately given, and discriminatory reactions, of course, are public and communicable.

Physicalism

As thoroughgoing empiricists the Logical Positivists hold that all meaningful scientific propositions are derived from experience. More precisely, all such propositions are reducible to *protocol-sentences*—sentences relating to the simplest elements of experience. This notion, I take it, is equivalent to the operationist's view that complex propositions are shown to be meaningful when they can be reduced to simpler propositions for which there are operational tests. The simplest propositions of all would be those relating to elementary discriminations. Now, if all scientific propositions are reducible in this fashion, including propositions expressed in what is called *physical language,* it must follow that *all* propositions are translatable into the physical language—a language similar to that of contemporary physics. This is the thesis of Physicalism.[5]

Physicalism was christened by Neurath (cf. 45). Contrary to what the name suggests, it is not a metaphysical doctrine asserting that everything is physical, for such a proposition can have no testable meaning. It is, on the other hand, a thesis relating to language: The physical language is a universal language of science and the

[5] This is a somewhat oversimplified statement of Physicalism. Furthermore, Carnap (21) has recently introduced extensive qualifications and changes into the original views of the Vienna Circle regarding the relation of the various 'languages' of science. His reasons for preferring the physical to the psychological language (pp. 9 ff.) do not appear to me to be binding, especially if the psychological language is made operational. If that is done, the choice becomes one based on convention or convenience. We could express all physics in psychological language, but that would be more traumatic to tradition than if we were to express all psychology in the physical language. The name Physicalism justifiably appeals to many as an unhappy designation, because it arouses prejudices by suggesting the primacy of a materialistic physics.

individual languages used in any subdomain of science can be equipollently translated into the physical language. Innocent as this assertion about language may appear, it is charged with far-reaching implications for psychology. In fact, the examples used to illustrate Physicalism make it appear that the doctrine was aimed directly against psychology—at least against the kind peddled by philosophers.

Physicalism makes it clear that the traditional but somewhat antiquated problem of psychophysical dualism is exclusively a problem of syntax. Using the common 'material mode' of speech we might say: To every psychical state there is a corresponding physical state of the body and the two are lawfully connected. Couched in this form, such a sentence is a veritable gold mine for pseudo problems. Physicalism would throttle these problems by saying: All sentences purporting to deal with psychical states are translatable into sentences in the physical language. Two distinctly separate languages to describe physics and psychology are therefore not necessary. And in this assertion we have Physicalism's denial of metaphysical dualism. It is the Logical Positivist's way of saying that psychology must be operational and behavioristic.

The philosopher, Hempel (27), calls this kind of psychology *logical behaviorism*. It differs from the primitive American stamp in that it does not prescribe that research shall be limited to stimulus-response connections. It is not, properly speaking, a theory *about psychology* at all, but only a logical theory about psychological sentences. The psychologist may study anything he pleases, but any verifiable psychological proposition he may utter is equivalent to some proposition in the physical language. An operationist would certainly agree to this notion. In fact, an operationist would point out that this view is correlative with his own dictum that any meaningful psychological proposition, even though it pertains to a toothache, is reducible to public, concrete operations.

The Unity of Science

How we get from Physicalism to the thesis of the *unity of science* is obvious indeed. If every sentence can be translated into the physical language, then this language is an all-inclusive language—a universal language of science. And if the esoteric jargons of all the separate sciences can, upon demand, be reduced to a single coherent language, then all science possesses a fundamental logical unity.

This idea of a unified basis for science, introduced into the Vienna Circle by the imaginative originality of Neurath, has launched a

whole new movement in scientific philosophy. The newly-begun *International Encyclopedia of Unified Science* is tangible testimony to the vigor and seriousness of the enterprise.[6] Annual congresses provide a forum where the thesis is developed (Fifth Annual Congress to be held at Harvard University, September 5–10, 1939); and out of this intellectual ferment there is emerging a substantial basis for an empirical and universal Science of Science. But before we inspect this newest of sciences—one which is obviously still warm in the womb of its philosophy-mother—let us look backward a few centuries.

How many men, since ancient Thales proposed that all is water, have dreamed the dream of a universal science is beyond a guess. The dream has taken many forms—mostly impracticable—for the history of science is a story of diversification and specialization proceeding almost geometrically with time. If there is unity in so much arborescence, where are we to find it? Certainly not in subject matter where differentiation is the rule. Perhaps, then, in method and logic.

In 1666 the twenty-year-old Leibnitz (2) dreamed his own dream about the unity of science and recorded it in *De Arte Combinatoria.* He himself called it a schoolboy's essay, but in it he proposed to create *"a general method in which all truths of reason would be reduced to a kind of calculation. At the same time this would be a sort of universal language or script, but infinitely different from those*

[6] Neurath (46) describes *unified science* as *encyclopedic integration.* The new "Encyclopedia" is to be constructed like an onion. The heart of the onion will be two introductory volumes consisting of twenty pamphlets, and in these volumes will be laid the foundations for a logical unity which will make possible future integration of scientific disciplines. The first layer of the onion enclosing the heart will be a series of volumes to deal with problems of systematization in special sciences, including logic, mathematics, the theory of signs, history of science, classification of the sciences, and the educational implications of the scientific attitude. Still outer layers will concern even more specialized problems. The encyclopedia will not be an alphabetical dictionary and its creators hope, quite piously, that it will not become a mausoleum but remain a living intellectual force.

At the present writing only three numbers (1, 2, and 5) of the "Encyclopedia" have appeared, but it is already clear that, although there is great community among the contributors, detailed unanimity is absent. As to the problem of unity in science, for example, Carnap finds as yet *no unity of laws* in science, but only *unity of language*; Lenzen finds a basis for unity in the fact that all science starts from experience; Neurath would get his unity by means of *encyclopedic integration*; Russel says the unity is essentially one of method; and Dewey hopes for unity by promulgating what he calls the scientific attitude.

projected hitherto; for the symbols and even the words in it would direct the reason; and errors, except those of fact, would be mere mistakes in calculation." How long would it take to create this logistic? Leibnitz thought a few chosen men could turn the trick within five years. But chosen men were not at hand and two centuries passed before the creation of a universal symbolic logic was even begun. Almost another century of labor has been needed to lay a foundation in logic and syntax so tangible that many men together could vision the unity of science.

Leibnitz though, if any single man, was father to the idea. He hoped for a universal logicalization of human thinking by means of a general calculus and a general terminology. He conceived a formal discipline to include a theory and art of forming signs to represent ideas and a general calculus giving a universal formal method of drawing consequences from the signs. Then, if two men were to find themselves in disagreement as to anything except matters of observation, they would settle their argument by calculating the right answer. Leibnitz' inspiration is perhaps not without its utopian aspect, but it cannot be denied that the modern logic of science has made progress towards Leibnitz' goal.

Perhaps our progress has not always been of the sort that would have delighted the boy of twenty, for metaphysics was no triviality in 1666. Today, however, it is clear that the unhappy symphonies of pseudo propositions that are metaphysics have all too frequently thwarted our efforts at clarification. Logical analysis has unmasked metaphysics; at least that is one of the boasted achievements of the recent philosophical movement. Opinion will probably never be unanimous on this issue, but disclosure of the empirically meaningless aspects of metaphysics is intimately bound to the other advances claimed by the Logical Positivists. By way of review at this point, these are some of the achievements of the modern movement:

1. It has been demonstrated that a unified language of science is possible. The syntax of this language is to be discovered by careful analysis of linguistic usage in science. And what unity there is in science is to be found in the unity of its logic and syntax.

2. Linguistic analysis has revealed the all-important distinction between the *formal* and the *empirical* aspects of science. Formal science consists of the analytic statements established by logic and mathematics; empirical science consists of the synthetic statements established in the different fields of factual knowledge.

3. The statements of logic and mathematics derive their validity from conventions, and, from the point of view of empiricism, they are materially empty and constitute a closed system of tautologies. Logic deals with language only—not with the objects of language. Likewise, mathematics deals with symbols—not with the objects which the symbols represent.

4. Empirical propositions have meaning when there exists a concrete procedure (a set of operations) for determining their truth or falsity. Empirical significance attaches only to testable or confirmable sentences.

5. What we have called the 'truth' of an empirical proposition is something which can never be absolute. Repeated tests of an object-sentence can add to its probability but never clinch its certainty. Induction, as Hume pointed out, is not a watertight method of proving anything empirical.

6. The notion that all scientific sentences are translatable into a common form—the physical language—requires of psychology a behavioristic approach. Psychology so conceived is called *behavioristics.*

These alleged achievements of the philosophers have been attained in the same spirit professed by the operationists: an empirical study of the actual doings of science-makers. Little wonder, then, that the two groups, although differing in emphasis, have arrived at substantially the same generalizations. Furthermore, these studies investigating the science-makers are the beginnings of a Science of Science. Like all other sciences, this one began before it was founded. Its founding and christening are of very recent date. They coincide with the harvesting of its first fruits.

The Science of Science

These first fruits of the Science of Science, it would appear, are the positive advances of operationism, of Logical Positivism, and of all who have looked seriously into the rules under which science is created. Except for these fruits, of which many are still green and some may even turn out to be wormy, the Science of Science comprises little more than an optimistic program. The fullest account of this program is supplied by C. W. Morris in his excellent essay on the "Foundations of the theory of signs" (42).

Morris is a philosopher at Chicago, and many will want to ask: What good is a science in the hands of philosophers? The obvious

retort is that all our major sciences passed their childhood in the mansion of philosophy and only after they had grown tough and empirical were they bold enough to desert the tender-minded parent. It may be that once again a band of curious men have turned up in some unsuspected corner a new science with which they will charm away a few hardy scholars and leave the parental mansion tenanted by the tender-minded.

Let us turn now to an outline of the scientific study of science. Morris calls it " Metascience " or " Scientific Empiricism." Morris is enthusiastically full of new terms; in fact, a difficulty with his account is that he is overly generous in his willingness to enrich our vocabulary. Much of his coinage, however, is choice and merits more extensive circulation. Morris defends the thesis that *it is possible to include without remainder the study of science under the study of the language of science, because the study of that language involves not merely the study of its formal structure but its relation to the objects it designates and to the persons who use it.* Language is a system of signs or symbols and the general science of signs is to be called *Semiotic.* Semiotic has a double relation to the other sciences: It is both a science among the sciences and an instrument of the sciences. It is not a ' superscience ' but rather a common science among the others. Every scientist at some stage of his work must embody his results in linguistic signs, and consequently he must be as careful with his linguistic tools as he is in designing his apparatus or in making his observations. In his enterprise, the scientist unites empiricism with methodological rationalism, and Semiotic studies how this marriage is consecrated.

The study divides itself into three dimensions or levels, which we shall discuss in turn:

1. Syntactics is the study of the relation of signs to signs.
2. Semantics is the study of the relation of signs to objects.
3. Pragmatics is the study of the relation of signs to scientists.

Syntactics refers to the formal disciplines commonly called logic, mathematics, and syntax, where the relation of signs to one another is *abstracted* from their relation to objects and to users or interpreters. At present this is the best developed branch of Semiotic, but in the field of the logical syntax of language there is still great labor to be done. The investigation of language from the syntactical point of view is at once both complex and fruitful. It has been possible accurately to characterize primitive, analytic, contradictory, and

synthetic sentences, and to show that many sentences which are apparently object-sentences (and so concern things which are not signs) turn out under analysis to be pseudo object-sentences which must be interpreted as syntactical statements about language. An astonishing number of the scientist's sentences are syntactical in this sense (see Appendix I). They are propositions without material content.

Ayer (1, p. 63) gives us a "striking instance" of the way in which propositions which are really linguistic are often expressed in such a way that they appear to be factual. At first glance, the proposition, "A material thing cannot be in two places at once," looks quite empirical, but critical inspection shows that "it simply records the fact that, as a result of certain verbal conventions, the proposition that two sense-contents occur in the same visual or tactual sense-field is incompatible with the proposition that they belong to the same material thing." The proposition, then, is a definition—it records our decision as to how we shall use the term "material thing." As this example suggests, the scientist frequently couches in the material idiom the propositions which he really intends as definitions, and thereby he tends unwittingly to generate pseudo problems out of his use—or misuse—of signs.

Of course, science is not the only activity in which we use signs. The artist, the musician, and the traffic cop are notable sign-users. What their various signs express or designate concerns semantics; what the effect of these signs is on society and the individual concerns pragmatics; but we can also inquire under what rules the signs are made, combined, and transformed, and that is syntactics.

Semantics refers to the rules determining under what condition a sign is applicable to an object or situation. Thus, the operational rule [7] laid down by Bridgman for determining the meaning of a term is, I take it, essentially a *semantical rule*. And the so-called "appli-

[7] In discussing operationism I have used the words *term* and *proposition*, *applicability* and *truth* (Stevens, 57, 58). In keeping with the spirit of Semiotic I ought perhaps to say that *terms* have *applicability* under semantical rules when the criteria governing their use are operational criteria. Then, sentences formed by combining these *semantically* significant terms into propositions are *empirically* significant (have truth-value) when their assertions are confirmable by means of operations. In other words, there is a justifiable distinction between the operational meaning of words and symbols (semantical significance) and the operational meaning of empirical propositions. I am not certain, however, that Morris would distinguish between empirical and semantical propositions in the same way.

cational definitions" used by the Logical Positivists to state when a term shall apply to an object come under this heading (cf. Blumberg and Feigl, 8). Within the study of these rules belong all the problems relating to the correlation between the signs which comprise a scientific treatise and the discriminable aspects of the physical world to which the signs are meant to apply. The simplest semantical rule is that governing an *indexical* sign. Such a sign designates what is pointed at at any instant. The denotation of the sign is based upon the operation of pointing, which in turn, of course, involves an act of discrimination. We have already noted that discrimination is the simplest and most basic operation performable.

Many of the problems of semantics belong to psychology. Morris sees in the experimental approach made possible by behavioristics great promise for determining the actual conditions under which certain signs are employed. Unfortunately, rules for the use of sign-vehicles are not ordinarily formulated by the users of a language; they exist, rather, as habits of behavior, and semantics wants to know what these habits are and how they come to be established. Many pertinent experimental studies have already been made by psychologists seeking the conditions of concept formation and judgments of similarity, but more are in order. Tolman's discovery of sign-gestalts functioning in the life of the rat discloses semantics among the rodents, and Lashley's effort to discover what range of patterns are considered equivalent by the rat when he uses them as signs for food directs attention to the problem of functional substitutivity (to use Professor Boring's term) among symbolic forms.

The game of chess is frequently suggested (cf. Carnap, 18, and Reichenbach, 51) as an example of a system of conventional formal rules applicable to concrete objects and situations. Perhaps at this point we can better illuminate Semiotic by examining this ancient pastime. First let us consider a set of signs. We shall use 3 groups of symbols: (1) the letters *a, b, c, d, e, f, g,* and *h*; (2) the numbers 1, 2, 3, 4, 5, 6, 7, and 8; and (3) certain other signs such as Kt, B, Q, K, etc. Next we shall set up conventional rules for manipulating these symbols by allowing only combinations in which 1 sign from each of the 3 groups appears, such as, for example, Kt *c* 4. This combination shall be transformable into other combinations, depending upon the first symbol, Kt. Thus:

$$\text{Kt } c\ 4 \longrightarrow \text{Kt } e\ 5.$$

But we shall not be allowed to write:

$$\text{Kt } c\ 4 \longrightarrow \text{Kt } d\ 5.$$

Now, when we have stated all the rules governing these signs, what do we have? Quite plainly, what we have is a formal system—a set of signs governed by syntactical rules. We are engaged in the pursuit of syntactics.

Anyone who is a chess player will have guessed by now that these syntactical rules were *abstracted* from the game of chess. The point is that we can abstract them in this way and study them with no reference to anything beyond themselves. On the other hand, we can use them as a 'model' to describe chess. In order to use them in this way we proceed to set up *semantical rules*. We say: Let the letters stand for the rows and the numbers for the columns of a chess board; let Kt stand for a particular small object (called a knight) which sits on a square of the board; then define Kt *c* 4 as equivalent to the statement that there is a knight on the square of coördinates *c* and 4; and define Kt *c* 4 ——→ Kt *e* 5 as equivalent to the statement that the knight is moved from *c* 4 to *e* 5. These semantical rules are statements about the use of language—they merely record our decisions as to how we shall use certain signs—and as semantical rules they are not empirical propositions. (This distinction between semantical and empirical statements was not made sufficiently explicit in operationism, but it needs to be stressed.)

We create an empirical statement as soon as we say that Kt *c* 4 is true, *i.e.* that there is, in fact, a knight on *c* 4, because this statement can be operationally verified. We can look to see whether our knight is there on *c* 4, or elsewhere. If the knight is on *c* 4 the statement is confirmed as true and if the knight is not on *c* 4 the statement is unconfirmed and is false. On the other hand, the statement 'Kt *c a*' can never be considered an empirical proposition, because this combination of signs violates the rules of syntax and is meaningless—it cannot be tested operationally.[8]

From our game of chess we can abstract still another dimension or aspect. We can ask: What is the relation of these rules to chess players? Is the game hard or easy? What is its place in society, etc.? Here we are broaching pragmatical questions.

Pragmatics, as a part of Semiotic, studies the relation of signs to scientists. Here belong the problems as to how the scientist, as a

[8] Note the similarity between the statement 'Kt *c a*' and Ayer's example discussed above. To say that a knight cannot be on *c* and *a* at the same time is very like saying that an object cannot be in two places at once. Both statements follow directly from the rules of our syntax and are therefore nonempirical sentences.

behaving organism, reacts to signs; how science, as a social institution, interacts with other social institutions; and how scientific activity relates to other activities. This, indeed, is the aspect of Semiotic most challenging to the psychologist. It is the problem of the interpretation of signs. What is their effect on the man who sees or hears them? How do they determine behavior? How are they used and abused in shaping human destiny? A nebulous problem, one might complain, and overwhelmingly complex. "Yes, but none the less real and pressing," must be the answer.

The term "pragmatics" obviously suggests the philosophy known as pragmatism. The word was deliberately chosen to be thus suggestive. (In Semiotic we should say that the *pragmatical* aspect of the word is one of suggestiveness.) Pragmatism, more effectively than ever before, directed attention to the relation of signs to their users and assessed this relation as an aid in understanding intellectual activities. Pragmatics, as part of Semiotic, pays tribute to the achievements of Peirce, James, Dewey, and Mead, but it must not be thought identical with pragmatism as a philosophy.

Both pragmatism and pragmatics agree that the interpreter of a sign-vehicle is an organism whose 'taking-account' of the sign consists in a *habit to respond* to the vehicle as it would to the thing designated by the sign. We thus find the problem of pragmatics cast in such a form that it can be handled by behavioristics—we deliberately avoid talking about the subjective effects of signs unless these effects are disclosed by public operations. Not only do we react to the signs appearing in sober scientific propositions, but our habits of response carry over to situations where signs obey neither semantical nor syntactical rules. We are often delighted by senseless jingles and moved to strong emotions by what analysis shows to be gibberish. In propaganda, where syntax is usually not violated, but where semantical relations are sometimes distorted, the pragmatical effects (the induction of some form of behavior) may be profoundly disturbing. Clearly, psychology has a stake in the solution of all these problems arising in pragmatics.

One more facet of this many-sided problem deserves our interest. What Morris calls *descriptive pragmatics* occurs when a sign used by a person is employed as a means of gaining information about the person. The psychoanalyst studies dreams for the light they throw upon the dreamer, not to discover whether there are actually any situations which the dreams denote. Likewise, we may study the statements of newspapers and politicians, not as empirical propo-

sitions, but for their ability to disclose the faction whose interest is being served by this form of propaganda. And in much the same spirit, the psychiatrist inspects the signs used by his patient in order to diagnose an abnormality. The pragmatical aberrations found among the psychoses are extremely illuminating, for occasionally a patient lets his system of signs displace completely the objects they once stood for; the troublesome world of reality is pushed aside and the frustrated fellow gets his satisfaction in the domain of signs, oblivious to the restrictions of syntactical and semantical rules. The field of psychopathology thus holds great promise as a place to apply Semiotic and discover some of its laws.[9]

There can be no doubt that in the realm of human behavior the concept of sign holds a key place. And if, as the pragmatists contend, mental phenomena are to be equated to sign-responses, psychology bears an intimate relation to the science of signs. The theory of signs—being the coördinated disciplines of syntactics, semantics, and pragmatics—is the core of a unified science. "Indeed," exclaims Morris (42), "it does not seem fantastic to believe that the concept of sign may prove as fundamental to the sciences of man as the concept of atom has been for the physical sciences or the concept of cell for the biological sciences."

Epilogue

That then, in all too brief review, is the manner in which the Science of Science has been staked out. Whoever would probe the making of science can learn all the answers by inspecting thoroughly the language of science. The investigator must remember, however, that *this language is an intersubjective (public) set of sign-vehicles whose usage is determined by syntactical, semantical, and pragmatical rules.* By making the Science of Science coextensive with the study of the language of science we have set spacious bounds to this field of inquiry—there is ample room for a variety of talents, and to bring

[9] Count Alfred Korzybski has written a bulky work called *Science and sanity* (Lancaster: Science Press, 1933), in which he contends that in the miseducation of our youth we teach them semantical rules based upon static Aristotelian classifications which they must then use in dealing with a fluid dynamic universe. Such semantical habits are enough out of tune with reality to drive many people crazy. Korzybski would cure the resulting insanity by renovating the patient's semantics. Whatever our opinion about this etiology and cure, it is plain that much of Korzybski's concern is with what Morris would call pragmatics—the effect of signs upon the users of signs.

all the diverse areas under cultivation will require coöperation among the specialties.

Three features of this lusty embryonic science stand out with particular prominence.

First, the rational and the empirical elements in science are disentangled and then reassembled according to a straightforward, workable plan. The formal, rational, analytic, *a priori,* deductive side of creative thinking, which has always been so dear in the hearts of James's " tender-minded," neither rules nor is ruled by the empirical, synthetic, *a posteriori,* inductive wing. Neither side can be called a usurper when both are understood, for they are not even in competition. Their union is achieved, not after the manner of Kant, who held out for a bastard hybrid which he called the " a priori synthetic judgment," but in conformity with the relation of sign to object.

Secondly, it is proposed that in our study of the science-maker we begin with the *products* of his activity—his finished propositions—rather than with his ' experiences ' or any other phase of his earlier behavior. This is a sensible place to begin. If we were to study the manufacture of any product, such as automobiles, we should probably find it useful first to ascertain what an automobile is and then to discover the conditions under which it comes into being. Science manufactures sentences, and we, as curious mortals, ask: What is a sentence and how is it made? The *complete* answer to this question is the Science of Science.

Thirdly, does it not appear that the Science of Science must go directly to psychology for an answer to many of its problems? Is it not also plain that a behavioristic psychology is the only one that can be of much help in this enterprise? A sign has semantical significance when an organism will react to it as it would to the object which the sign supplants. The psychologist works out the laws under which different stimuli evoke equivalent reactions. Signs, as stimuli, can be combined and utilized extensively in the control and direction of behavior, both individual and social. The entire activity of the scientist as a sign-using organism constitutes, therefore, a type of behavior for which behavioristics seeks the laws. If there is a sense in which psychology is the propaedeutic science (cf. Stevens, **58**), it is undoubtedly in its ability to study the behavior, *qua* behavior, of the science-makers.

Perhaps we are too close to this young Science of Science either to judge its value or see clearly how it came to be. We shall forego the value-judgment, since it would merely disclose the author's par-

ticular prejudice (already clear, no doubt), but an observation about the movement's immediate ancestry is not entirely out of order. It now appears, in retrospect, that the Science of Science emerged as the reasonable outcome of revolutions in the three major fields: physics, psychology, and philosophy. These revolutions occurred almost independently, but a general community of spirit among them led directly to extensive cross-fertilization. Operationism as a revolution against absolute and undefinable concepts in physics, behaviorism as a revolution against dualistic mentalism in psychology, and Logical Positivism as a revolution against rational metaphysics in philosophy were the three forces whose convergence into a common effort is effected by the Science of Science.

Finally, the purpose of this review has been to call the attention of those of us who are psychologists to the critical principles involved in scientific method as evolved in recent scientific and philosophic movements. We have had little to say concretely about psychology or its facts, and undoubtedly many will be impatient with so much nonexperimental discourse. "Who cares about philosophy?" they will say. "What matters is the product of the laboratory." While such robust empiricism is admirable, we must ask the indulgence of these tough minds. We must ask them to bear with us while we inspect our logical tools as carefully as we do our other apparatus. And we must ask them to weigh the implications for psychology of this statement by Quine, the logician (**48**):

> "The less a science has advanced the more its terminology tends to rest upon an uncritical assumption of mutual understanding. With increase of rigor this basis is replaced piecemeal by the introduction of definitions. The interrelationships recruited for these definitions gain the status of analytic principles; what was once regarded as a theory about the world becomes reconstrued as a convention of language. Thus it is that some flow from the theoretical to the conventional is an adjunct of progress in the logical foundations of any science."

APPENDIX I

A Case Study in Language

To a greater extent than he probably realizes the scientist indulges in nonempirical discourse. It is necessary that he should, for he must help us to understand him by telling us how he intends to use his words and symbols. Most often, however, he so informs us by a method that is more implicit than explicit. He does not tell us which of his sentences are stating syntactical and semantical rules, but he hides these rules in sentences which have the outward appearance of material or empirical

propositions. Not always does this practice make trouble for us, although sometimes the material mode of expression leads to arguments about pseudo problems which can properly be settled only by our agreeing to a uniform language-convention. At any rate, it is an interesting and instructive exercise occasionally to inspect our scientific books with a view toward segregating the syntactical from the empirical assertions. As an essay at such an exercise, let us examine a passage from J. F. Brown's important and scholarly work, *Psychology and the social order* (New York: McGraw-Hill, 1936). I shall present the passage (quoted from pp. 238–239 by permission of the author and publisher), together with a tentative analysis of the nature of each sentence. This analysis may or may not be the correct one—it is always dangerous to take a passage out of context or to neglect earlier statements by the author—but I give it more to illustrate a method than to prove anything about Professor Brown's propositions.

Passage	*Kind of Sentence*
"We must first combat the usage of the term personality as a quantitative concept.	*Syntactical:* states that the word personality is not a number-word, or cannot be modified by number-words.
We speak popularly of this person as having a 'great deal of personality,' of that person as having 'but little.'	*Empirical:* asserts something about our speech-behavior which is capable of operational confirmation or refutation.
It is true that the individual traits (aspects) of personality vary, but	*Syntactical:* aspect-words are terms which can be modified by number-words.
everyone has quite the same amount of personality.	*Syntactical:* personality is a unit-word.
Personality hence becomes a qualitative rather than a quantitative aspect	*Syntactical:* personality is an expression of kind rather than number.
of nature and may be defined *as the pattern or arrangement of the individual traits.*	*Syntactical:* personality is a class-word and includes traits.
We shall see later that	*Empirical:* a statement about the book itself—confirmable.
personalities vary in the degree of structuralization, but	*Syntactical:* personality is a class-word including structures. (This type of sentence might, of course, be *empirical,* provided the terms personality and structuralization have been *independently* defined elsewhere.)
this variance in degree of structuralization is more concerned with pattern than with amount.	*Syntactical:* structuralization is more a relation-word than a number-word.
Secondly, we as psychologists are not interested in whether the personality is 'good' or 'bad.'	*Empirical:* states something about us: we are indifferent about the syntactical relation between the word "personality" and value-words.

Scientifically we should be able to account for both sorts on the basis of our principles.	*Syntactical:* the relation between personality-words and value-words is implicit in our syntactical rules.
Thirdly, we must combat the idea that the individual personality is a constant aspect of the individual.	*Semantical:* the same personality-word does not always go with the same individual.
One's personality changes as the pattern of traits changes.	*Syntactical:* personality is a relation-word.
John Smith . . . has not, from the cradle to the grave, the same definite and precise personality in the scientific sense of the word.	*Semantical:* different personality-words apply to John Smith at different times.
When a normal individual goes insane, his personality changes.	*Semantical:* when a person acquires a psychosis we apply a new personality-word. (This type of statement would not be semantical provided we had *previously* given *independent* definitions to insanity and to personality, for then we should have an empirical proposition of the 'if-then' form.)
When he changes his political affiliations, his personality likewise changes.	*Semantical:* when a person changes politics we apply a new personality-word.
Marriage or divorce or parenthood in its turn brings about personality changes."	*Semantical:* these are additional rules for applying different kinds of personality-words.

We see, therefore, that most of this passage deals with the relation of the word "personality" to other words (syntactical rules) or to situations (semantical rules). These rules are essentially conventions and can be neither proved nor disproved by appeal to experiment or observation.

Most of these propositions are presented in what is sometimes called the material mode, and the brief paraphrase in the right-hand column is more nearly in what may be called the formal mode. The paraphrase is not adequate in every case to carry the full significance of the original proposition, but presumably it could be made adequate if necessary. The advantage of the formal mode is that it more readily reveals contradictions or deficiencies among our syntactical and semantical rules.

APPENDIX II

A Note on the Hypothetico-deductive Method

Those who contend that science should be pursued in the manner of Galileo rather than of Aristotle advance as the distinctive feature of Galileo's procedure the hypothetico-deductive method. Under this method we set up hypotheses from which we deduce consequences, and these we test by experiment. There can be no doubt that this is the method of science at its best. How, then, does this method concord with the picture of science presented by operationism?

The scientist fits a formal model (language, mathematics, logic) to his observations. He does more, however, than merely set down semantical rules so that his model will provide a passive description of what he has done in his laboratory. He tries to select or create a model which has a set of rules governing the manipulation of the signs or elements of the model, and he does this in order that he shall be able to exploit the model and arrive at new, testable propositions.

In terms of hypotheses and deductions the procedure is this: We set up an array of symbols in the form of an equation or proposition (hypothesis). By means of semantical rules we relate these symbols to observable events. Then, by means of the syntactical rules governing our system of symbols, we proceed to deduce; that is to say, we apply our rules of transformation and convert our original equation or proposition into others consistent with it. We then relate the terms of these new propositions to observable events by means of semantical rules, and having done this, we are in a position to confirm by experimental observation the results of our deductions.

The point to be noted is that the process of deduction is carried out at the formal level. A deduction consists in a transformation of a sentence in accordance with syntactical rules. When our hypothesis is an equation and our syntactical rules are the laws of algebra, there can be no doubt that the procedure is as outlined here. When our hypothesis is a sentence in English, where the rules of syntax are less explicitly stated, we often make deductions without being fully aware of the 'logic' we are using. Nevertheless, if we are consistent, we are using a logic of some sort, and it is the carrying out of the rules of this logic which is the process we call deduction.

Clearly, then, the hypothetico-deductive method depends for its effectiveness not only on our ability to hit upon good hypotheses (a matter sometimes of hunch or inspiration or chance), but also upon the existence of a formal calculus or language whose explicit rules of transformation make effective deductions possible. The scientist should have as large a concern for the syntax of his language and the rules of his mathematics as he has for the equipment of his laboratory.

(It will be interesting, in this connection, to see what effect on the hypothetico-deductive method in biology will eventually result from Woodger's effort (**66**) to employ a more rigorous logical syntax in the description of biological phenomena, and from Lewin's effort (**30a**) to create a new formal system—"hodological space" or path geometry—for the description of the "life space" of behaving individuals.)

BIBLIOGRAPHY

In the hope that an annotated bibliography will prove more useful than a bare list of references, I have tried to indicate for some of these works the particular features which recommend them for consideration in connection with the present review. Not all of these features have been dealt with in the text and to that extent some of the following comments are supplementary to the review.

1. AYER, A. J. Language, truth and logic. London: Gollancz, 1936.

This is a medium-sized book of more than medium quality by a young English disciple of the Vienna Circle. Ayer went to Vienna for a couple of years when he was twenty-two, and he wrote this book before he was twenty-five. In the course of 248 pages, he gives a simple and readable account of the rightful rôle of philosophy as the Logical Positivist sees it. He disposes in rather cavalier fashion of metaphysics and of the assumed right of rational, deductive procedures to legislate for science. He distinguishes sharply between the *formal* methods of mathematics and logic, where only tautologous statements are possible, and the *empirical* methods of science, where all inductive propositions are "hypotheses" having one or another degree of probability.

2. BELL, E. T. Men of mathematics. New York: Simon & Schuster, 1937.

A delightful revelation of the humanity behind mathematics. It is interesting to trace in this work the progress of mathematics, not only in its concrete achievements, but also in its growing appreciation of itself as an essentially tautologous system resting on a set of fundamental conventions or postulates. The ancients tried to check their mathematics against nature; the moderns evolved so much mathematics (algebraic ideals, abstract spaces, much of number-theory, generalized geometries, etc.) which has no known application to the observable world that they had to recognize its basis in convention rather than its ability to picture empirical nature.

3. BENJAMIN, A. C. An introduction to the philosophy of science. New York: Macmillan, 1937.

4. BENJAMIN, A. C. The operational theory of meaning. *Phil. Rev., N. Y.*, 1937, **46**, 644–649.

Here are listed certain advantages to philosophy of operational criticism.

"The operational theory is essentially methodological rather than metaphysical." The definition of *operation* is extended by this author to include what he calls both extensional and intensional operations.

5. BENTLEY, A. F. The positive and the logical. *Phil. Sci.*, 1936, **3**, 472–485.

Although agreeing with the announced aims of Logical Positivism, Bentley finds some of the works of its prophets to be seriously unworkmanlike. He finds instances in which they are neither logical (consistent) nor positive (agreeing with the methods of recognized science).

6. BENTLEY, A. F. Physicists and fairies. *Phil. Sci.*, 1938, **5**, 132–165.

Bentley wonders why physicists are so foolhardy as to risk excursions into "epistemologizing." "The kind of psychology that the physicist is most apt to get his fingers on deals mostly with fairies, sprites and spooks." This paper is a review of the experiences of operationism among physicists. The author begins with Bridgman, who at least began with a sound idea. Margenau criticized Bridgman, and Bentley criticizes Margenau. Then Lindsay criticized Bridgman, and Bentley criticizes Lindsay. And then Bridgman wrote his book on *The nature of physical theory*, and Bentley criticizes Bridgman. There are, nevertheless, many interesting points made in the course of this round of criticism.

7. BILLS, A. G. Changing views of psychology as science. *Psychol. Rev.*, 1938, **45**, 377–394.

Here we are treated to an interesting address pointing out both the achievements and dangers of psychology's attitude toward method and principles.

8. BLUMBERG, A. E., & FEIGL, H. Logical positivism. *J. Phil.*, 1931, **28**, 281–296.

This is an article introducing Logical Positivism to American scholars. It is a good statement of the *early* position of the Vienna Circle.

9. BOAS, G., & BLUMBERG, A. E. Some remarks in defense of the operational theory of meaning. *J. Phil.*, 1931, **28**, 544–550.

These authors coin the word "operationalism" which they attribute to Bridgman. Bridgman, incidentally, dislikes calling the operational method an 'ism,' but the word "operationism" is probably too useful to be suppressed.

10. BORING, E. G. Temporal perception and operationism. *Amer. J. Psychol.*, 1936, **48**, 519–522.

Here we see how a traditional problem in psychology turns out to be specious in the light of operational criticism.

11. BRIDGMAN, P. W. The logic of modern physics. New York: Macmillan, 1928.

In this book appears the original statement of the operational view and an elaborate application of it to specific concepts in physics. It is this important and able book which establishes Bridgman as the father of the operational movement in physics.

12. BRIDGMAN, P. W. A physicist's second reaction to Mengenlehre. *Scripta math.*, 1934, **2**, 3–29.

In this article Bridgman attempts to apply operational criticism to class-theory: "Mathematics has meaning only in so far as it is amenable to the same sort of an operational control that has been found necessary in physics." Although this paper contains many important contributions to operationism, we must criticize its occasional failure to make an adequate differentiation between *formal* and *empirical* disciplines. Bridgman would seem to support the very questionable view that deductive propositions are not fundamentally different from inductive propositions.

13. BRIDGMAN, P. W. The nature of physical theory. Princeton: Princeton Univ. Press, 1936.

In these lectures operationism (a word Bridgman never uses) is expounded and amplified. Here we encounter excursions into psychology and epistemology and the outcome is perhaps not so happy as that of *The logic of modern physics.*

14. BRIDGMAN, P. W. Operational analysis. *Phil. Sci.*, 1938, **5**, 114–131.

This article is aimed principally at answering some of the criticisms which the author has previously encountered.

15. BRUNSWIK, E. Psychology as a science of objective relations. *Phil. Sci.*, 1937, **4**, 227–260.

Brunswik reviews the problem of how the organism deals with its environmental world, how it exhibits the "constancy phenomenon," and

how it "attains its objects." He shows how many of the older problems of psychology turn out to be pseudo problems when we make the proper operational shift in their definition. This important paper is handicapped, however, by an involved style of English.

16. BURES, C. E. The concept of probability. *Phil. Sci.*, 1938, **5**, 1–20.

This is an excellent critical survey of recent contributions to the concept of probability. I call attention to it here because of the rôle that probability seems destined to play in our notions of the truth-value of empirical propositions (cf. Reichenbach, **51**). After reviewing the other major theories Bures summarizes the case for the frequency interpretation of probability and points out its basic importance.

17. CAMPBELL, N. R. Physics: the elements. Cambridge: Univ. Press, 1920.

This has long been the standard work on the logic and the operational foundation of measurement in science. Part II, dealing with measurement, numbers, dimensions, errors, etc., is still of first importance. Part I, however, dealing with the nature of laws, theories, measuring, and truth, contains some statements which are candidates for revision. Nevertheless, it is impressive to note by how much this large and careful book anticipated the views and arguments of those interested in operationism, Logical Positivism, and the Science of Science generally.

18. CARNAP, R. On the character of philosophic problems. *Phil. Sci.*, 1934, **1**, 5–19.

This, the first paper appearing in the *Philosophy of Science*, gives a brief statement of the thesis that philosophy is the logic of science and proceeds to demonstrate the differences between the two modes of speech "*inhaltliche und formale.*"

19. CARNAP, R. Philosophy and logical syntax. London: Kegan Paul, 1935.

This tiny book contains the content of some lectures delivered at London. Carnap here explains the method of philosophizing used by the Vienna Circle: the "syntactical analysis of scientific language." He also discloses some of the implications of this method, such as Physicalism and the unmasking of metaphysics.

20. CARNAP, R. Les concepts psychologiques et les concepts physiques sont-ils foncièrement différents? *Rev. Synthèse*, 1935, **10**, 43–53.

Carnap examines the proposition, "Monsieur A est en colère," in order to show that it can be translated into a "physical language." He presents an interesting analogue in the form of a proposition stating: "Dans ce conducteur passe maintenant un courant d'intensité 5." There is a class of at least a half-dozen ways of verifying this latter proposition, and sentences based on any one of these ways can be translated into sentences based on any other. Similarly there is a whole class of methods for verifying the proposition about anger, and the corresponding propositions are translatable in a manner that is equipollent (*i.e.* reciprocally deducible).

21. CARNAP, R. Testability and meaning. *Phil. Sci.*, 1936, **3**, 419–471; 1937, **4**, 1–40.

Here Carnap treats the problems of confirmation, testing, and meaning, all of which rest upon some degree of testability or confirmability, or both. We are free to choose our criteria in these matters. The

notion of verifiability, if by verification we mean a *complete* and definitive establishment of truth, must be replaced by the notion of *degrees* of confirmability. A sentence is testable if we know a *method* for testing it; it is confirmable if we know under what conditions the sentence *would be* confirmed, *i.e.* how our experiment would have to turn out. A sentence may therefore be confirmable without being testable.

This long article (published in two parts in separate issues of the journal) goes on to a logical analysis and then to an empirical analysis of confirmation and testing. The last section deals with the construction of a language system. Throughout this paper we find frequent modifications of Carnap's earlier views.

22. CARNAP, R. Logical syntax of language. London: Kegan Paul, 1937.

This book is a translation of what is probably Carnap's most important work to date. Considering language as a calculus, the author sets out to systematize its rules or syntax and thereby provide the tools for working out the logic of science. The difference between the *material* and the *formal* mode of speaking is set forth with interesting examples of how it happens that many sentences which appear to be material (empirical) turn out under analysis to be syntactical (formal). These 333 densely packed pages deserve careful study.

23. DINGLE, H. Through science to philosophy. Oxford: Clarendon Press, 1937.

Dingle gives us a lively account of recent developments in the Philosophy of Science, but his criticisms of operationism appear definitely tender-minded.

24. EINSTEIN, A. On the method of theoretical physics. *Phil. Sci.*, 1934, 1, 163–169.

Einstein tells us that if we would know the method used by theoretical physicists we must not listen to what they say they do, but examine their achievements. He further shows how these physicists go about fitting a formal model to their observations.

25. FEIGL, H. The logical character of the principle of induction. *Phil. Sci.*, 1934, 1, 20–29.

Feigl holds that the principle of induction is neither analytic nor synthetic: "It is, rather, the principle of a procedure, a regulative maxim, an operational rule." It is something like a habit. This is a pragmatic view.

26. FEIGL, H. Logical analysis of the psycho-physical problem. *Phil. Sci.*, 1934, 1, 420–445.

In this article, the distinction is made between a language of data and a language of constructs. (For a criticism of this dichotomy see Stevens, 57.)

27. HEMPEL, C. G. Analyse logique de la psychologie. *Rev. Synthèse*, 1935, 10, 27–42.

This paper contains one of the best statements available of the implications for psychology of the thesis of Physicalism. Logical analysis of the language of science demonstrates, according to Hempel, that all meaningful psychological propositions can be translated into propositions containing no "psychological concepts." "La psychologie fait partie

intégrante de la physique." Any distinction between physics and psychology must be made solely for practical reasons.

28. HUME, D. Enquiries concerning the human understanding and concerning the principles of morals. (2nd ed.) Oxford: Clarendon Press, 1902.

29. JAMES, W. Pragmatism. New York: Longmans, Green, 1914.

James's popular statement of pragmatism done in the inimitable Jamesian manner.

30. KANTOR, J. R. The operational principle in the physical and psychological sciences. *Psychol. Rec.*, 1938, **2**, 3–32.

Kantor here develops his brand of operationism. He is unhappy about the latent subjectivism in previous versions of it, especially that of Stevens. He wants complete 'objectivity' but tries to get it by invoking what would appear to be essentially a realism. Apart from this possible defect, this paper is rich in valuable applications of the operational attitude.

30a. LEWIN, K. The conceptual representation and the measurement of psychological forces. *Contr. psychol. Theor.*, 1938, **1**, No. 4, 1–247.

Lewin is looking for a formal model capable of representing the human individual in a social environment. Since Euclidean geometry is too specialized and restricted to depict forces and movements in the social field, Lewin turned to the most generalized of geometries—topology. This geometry, however, is so general and its rules so scanty that it is somewhat barren of predictive power when applied to the social field. In order to provide a geometry intermediate between topological and Euclidean, Lewin has, in the present work, developed what he calls hodology. Hodology is the geometry of paths—a geometry in which distance and direction are defined, but not in metrical terms. As a formal model applicable to the "life space" of the individual, hodology holds great promise for psychology.

31. LINDSAY, R. B. A critique of operationalism in physics. *Phil. Sci.*, 1937, **4**, 456–470.

Here is where Lindsay accuses operationism of the kind of "particularism" which would "imply the abandonment of the method of theoretical physics."

32. LINDSAY, R. B., & MARGENAU, H. Foundations of physics. New York: Wiley, 1936.

An important advanced textbook on physics in which the authors do us the great service of giving us not only formulas but a critical discussion of their meaning, justification, and limitations. Here we find the "text" which Bridgman claims should go with every mathematical formula.

33. LUNDBERG, G. A. Quantitative methods in social psychology. *Amer. sociol. Rev.*, 1936, **1**, 38–54.

A plea is made for an operational rather than an intuitional approach to the problems of the social sciences.

34. MALISOFF, W. M. The universe of operations (a review). *Phil. Sci.*, 1936, **3**, 360–364.

This is an interesting critical review of Bridgman's *The nature of physical theory.*

35. MARGENAU, H. Causality in modern physics. *Monist*, 1931, **41**, 1–36.

Margenau, in the course of an extensive examination of the logic and meaning of causality as it appears in modern physics, makes the observation that operationism would, if carried to its consequences, land us in the midst of discrete concepts lacking logical coherence.

36. MARGENAU, H. Methodology of modern physics. *Phil. Sci.*, 1935, **2**, 48–72; 164–187.

A heavy, but able, inquiry into the problem of abstractness in modern physics.

37. MCGEOCH, J. A. Learning as an operationally defined concept. *Psychol. Bull.*, 1935, **32**, 688.

38. MCGEOCH, J. A. A critique of operational definition. *Psychol. Bull.*, 1937, **34**, 703–704.

McGeoch raises two pertinent questions for operationism: that of its ability to deal with qualitative discrimination and that of the operational definition of operations.

39. MCGREGOR, D. Scientific measurement and psychology. *Psychol. Rev.*, 1935, **42**, 246–266.

A careful and definitive consideration of the problems and the logic of measurement in psychology. McGregor worked in close collaboration with Professor Boring in producing this paper. The discussion demonstrates how the meaning of measurements rests upon the operations performed in measurement.

40. MENGER, K. The new logic. *Phil. Sci.*, 1937, **4**, 299–336.

All that the mathematician does "is to deduce propositions by certain methods (which must be formulated) from certain initial propositions (which must also be formulated). There are a variety of ways to choose both the methods and the initial propositions." The mathematician, as a mathematician, cannot say what is the relation of these propositions and methods to so-called reality.

41. MORRIS, C. W. Scientific empiricism. *Int. Encycl. unif. Sci.*, 1938, No. 1, 63–75.

This is a brief statement of the attitude of scientific empiricism, which embraces at once radical empiricism, methodological rationalism, and critical pragmatism. It is the application of this point of view to science itself that, according to Morris, generates metascience, or the Science of Science.

42. MORRIS, C. W. Foundations of the theory of signs. *Int. Encycl. unif. Sci.*, 1938, No. 2, 1–59.

In this article Morris defines the scope and aims of the science of Semiotic, or the science of signs. The study of the language (signs) used by science is the best approach to the Science of Science.

This paper climaxes ten years of revolution in science and philosophy.

43. NAGEL, E. Some theses in the philosophy of logic. *Phil. Sci.*, 1938, **5**, 46–51.

An address in which Nagel points out the growing scientific outlook among philosophers.

44. NEURATH, O. Historische Anmerkungen. *Erkenntnis*, 1930, **1**, 311–314.

The author gives us an orientation toward Logical Positivism in terms of its antecedent thinkers and its present near relations.

45. NEURATH, O. Physicalism: the philosophy of the Viennese Circle. *Monist*, 1931, **41**, 618–623.

This paper gives a brief, but somewhat discursive, statement of the physicalist doctrine whose name is due to the inventiveness of Neurath.

46. NEURATH, O. Unified science and its encyclopedia. *Phil. Sci.*, 1937, **4**, 265–277.

Neurath, who appears to be godfather to the new *International Encyclopedia of Unified Science*, here describes its purpose, background, and plan of development.

47. POINCARÉ, H. The foundations of science. New York: Science Press, 1913.

"All the scientist creates in a fact is the language in which he enunciates it."

48. QUINE, W. Truth by convention. In *Philosophical Essays for Alfred North Whitehead*. New York: Longmans, Green, 1936. P. 90.

49. RASHEVSKY, N. Foundations of mathematical biophysics. *Phil. Sci.*, 1934, **1**, 176–196.

This, and other articles by Rashevsky, are interesting in their effort to demonstrate what can be gotten by an application of classical mathematics to biological and psychological problems. This work should be compared to that of Woodger, who applies a somewhat different logical model to similar problems. It is perhaps too early to judge the relative fruitfulness of these two approaches. For a more detailed bibliography of Rashevsky's work see *Psychometrika*, 1936, **1**, 1–26.

50. RASHEVSKY, N. Physico-mathematical methods in biological and social sciences. *Erkenntnis*, 1936, **6**, 357–365; and Mathematical biophysics. Chicago: Univ. Chicago Press, 1938.

Rashevsky here outlines his purpose and method quite explicitly. What he attempts to do is "to develop mathematical biology as a *rational* theoretical science, according to patterns suggested by theoretical physics."

51. REICHENBACH, H. Experience and prediction. Chicago: Univ. Chicago Press, 1938.

Professor Reichenbach, formerly of Berlin, then of the University of Istanbul, and now in the United States, has brought together in this vital work the fruits of his years of work in philosophy, logic, and mathematics. He amplifies, extends, and on some points revises the philosophical outlook of those who, like the Logical Positivists, take a scientific attitude toward the traditional problems. He is interested in uniting "the empirical conception of modern science and the formalistic conception of logic," and he makes a clear restatement of the secured achievements of those interested in this same movement. He reconsiders these advances, however, in the light of the concept of *probability*. Probability has long been Reichenbach's forte, and here he applies it to the central problems of scientific induction. Empirical propositions always have a degree of probability which is less than certainty. Furthermore, probability has meaning only when interpreted as a relative frequency (contrary to Keynes' view). These two notions can be harmonized, says Reichenbach. (Read Chapter 5 to see how he achieves this harmony.) Chapter 5, incidentally, contains a résumé of his impor-

tant and well-known work "*Wahrscheinlichkeitslehre*" (Leiden, 1935).

The earlier chapters deal with meaning, impressions, constructs, etc. Especially to be recommended is his use of the game of chess to illustrate the three predicates of propositions: their meaning (verifiability), their truth-value (operational test), and their predictional value (probability or "weight").

52. Rosenzweig, S. Schools of psychology: a complementary pattern. *Phil. Sci.*, 1937, **4**, 96–106.

53. Schlick, M. De la relation entre les notions psychologiques et les notions physiques. *Rev. Synthèse*, 1935, **10**, 5–26.

An examination of the relation between psychological and physical concepts in the light of the thesis of Physicalism.

54. Seashore, R. H., & Katz, B. An operational definition and classification of mental mechanisms. *Psychol. Rec.*, 1937, **1**, 3–24.

55. Somerville, J. Logical empiricism and the problem of causality in social sciences. *Erkenntnis*, 1936, **6**, 405–411.

First of all, the author points out that the social sciences are to be included in the unity of science on an equal footing with the natural sciences. He then argues that "this unity is organic, and is to be found in the criteria, method, and language of science."

56. Stevens, S. S. The operational basis of psychology. *Amer. J. Psychol.*, 1935, **47**, 323–330.

The contents of this and the two following papers have been outlined in the body of the present paper.

57. Stevens, S. S. The operational definition of psychological concepts. *Psychol. Rev.*, 1935, **42**, 517–527.

58. Stevens, S. S. Psychology: the propaedeutic science. *Phil. Sci.*, 1936, **3**, 90–103.

59. Struik, D. J. On the foundations of the theory of probabilities. *Phil. Sci.*, 1934, **1**, 50–70.

The conflicting claims of the "schools" of probability (due to different synthetic judgments) mean for Struik that "the existence of a mathematical theory of probabilities remains a kind of miracle, as esoteric to the further domain of natural science as the resurrection of the dead."

60. Tolman, E. C. Purposive behavior in animals and men. New York: Appleton-Century, 1932.

This well-known work is an interesting attempt to give explicit operational definition of terms ordinarily considered 'mental.'

61. Tolman, E. C. An operational analysis of 'demands.' *Erkenntnis*, 1936, **6**, 383–390.

A demand (desire, wish, purpose, need, motive) is a "constructed" variable in psychology and it can be operationally defined. Demands are of three kinds: primary appetite demands, primary aversion demands, and secondary demands. All three types can be demonstrated in the rat, where, as Tolman shows, operational definition is easy. He is, as a matter of fact, uneasy about the utility of verbal report:

"My motto for the present is:

Rats, not men

Gross behavior, not verbal reports."

(Fortunately, all psychologists are not so ready to sell out to the rodents.)

62. TOLMAN, E. C. Operational behaviorism and current trends in psychology. *Proc. Twenty-fifth Anniv. Celeb. Univ. S. Calif.*, 1936, pp. 89–103.

63. WATERS, R. H., & PENNINGTON, L. A. Operationism in psychology. *Psychol. Rev.*, 1938, **45**, 414–423.

In this critical review, the authors object to Bridgman's principle on these grounds: (1) the principle lacks novelty; (2) it does not guarantee unanimity in psychology; (3) it leads to multiplication of the number of concepts; and (4) it unnecessarily restricts the scope of operational definitions.

64. WEINBERG, J. R. An examination of logical positivism. London: Kegan Paul, Trench, Trubner, 1936.

This is, at present, the most extensive critical review of the work of the Vienna Circle. Weinberg begins with the theories of Wittgenstein and traces the development of Logical Positivism through Physicalism. He points out the various difficulties encountered by the movement at successive stages and shows how these difficulties were met or failed to be met.

65. WITTGENSTEIN, L. Tractatus logico-philosophicus. New York: Harcourt, Brace, 1922.

66. WOODGER, J. H. The axiomatic method in biology. Cambridge: Univ. Press, 1937.

Here is a book whose aim must command the attention of all psychologists who have followed with interest the efforts of certain workers, like Lewin and Hull, to adapt a formal scheme to the representation of psychological data. This book is an experiment in the application of a type of logistic to biology. Woodger, with the help of logicians Carnap and Tarski, tries to "provide an exact and perfectly controllable *language* by means of which biological knowledge may be ordered." He believes that if we have a perfect language we need not dispute—we need only calculate and experiment. The symbols of the biological calculus developed in this book are those of modern logistic. The impression is almost inevitable that Woodger's approach, beginning with the problem of an exact 'language' and then applying that 'language' to biology, is one that could be used in psychology. Perhaps this approach to Hull's "psychological system" would make his efforts even more fruitful. (Since first writing this note, I find that Woodger has already made a tentative effort to "formalize" Hull's system. (Woodger, J. H. The formalization of a psychological theory. *Erkenntnis*, 1938, 7, 195–198.)

SUGGESTION AND SUGGESTIBILITY[1]

A BIBLIOGRAPHY

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In spite of excellent bibliographical services available to psychologists and other professional groups such as political scientists and sociologists, a research worker in any of these groups must spend an exorbitant amount of time tracking down literature bearing upon his problem. Titles of articles often hide rather than reveal the methods used, the subjects employed, and the concepts derived. It was considered desirable, therefore, to prepare an analytical index for several different topics broadly conceived as falling within the area of social psychology. This is the first in a series under preparation. Although an attempt has been made to include all pertinent references published in English, no claim can be made for completeness. The field of hypnosis has been avoided except where it definitely concerns comparisons or contrasts with suggestibility. If the bibliographies prove useful to colleagues, and if the generosity of the Federal Government can be extended to provide workers with appropriate skill, it might be feasible to analyze articles in foreign journals. As new bibliographies are prepared, they will be offered for publication.

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[1] This bibliography has been made possible by aid from the Federal Government. It is part of the work done as Subproject No. 240 of Official Project No. 461–71–3–36 and No. 50576–85090 of the State of Minnesota under the Works Progress Administration.

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INDEX

AUTOMOBILE DRIVERS CAN BE IMPROVED

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In an article in this journal,[1] Harry M. Johnson and Percy W. Cobb question the educational value of drivers' clinics as sponsored independently by Slocombe, Lauer, and the writer on the basis of published evidence. Speaking for myself, I have confidence in the possibility of rehabilitating drivers who have had accidents, in spite of Johnson's unbounded pessimism that they are "born to crash," and that the only way to cure them is to drastically and permanently remove perhaps 5%, or some 2,000,000, drivers from the road.[2] Since, however, facts speak stronger than words, I shall summarize here 2 investigations bearing on the problem of the possibility of re-educating drivers.

In recent articles,[3, 4] Forbes and the author have summarized the educational results of a driver clinic administered to a number of accident drivers who were called in by the Manchester, New Hampshire, Police Department. A total of 180 names were picked from the accident file of drivers who had had 2 or more accidents during the preceding 15 months. On account of limited time at our disposal we were able to test and interview only 101 of these drivers. The 79 other drivers received the benefit of the educational work of a Safety Campaign. The 2 tables below indicate the improved driving record of the drivers attending the clinic as compared with other equally bad drivers not attending the clinic, both from the standpoint of total accidents (Table I) and drivers involved (Table II).

Another article[5] reviews the educational effects of a driver clinic (involving test and educational interview) on the accident records of 163 bus drivers of the Boston Elevated Railway. Table III summarizes the accident records of those drivers for the year following and the 3 years preceding their clinical tests. It will be noted that there was a *reduction of 28% in chargeable accidents* of drivers during the year subsequent to attending the clinic. *This improvement took place in spite of the fact that the Boston Elevated accident rate for all drivers was 5% higher during this period than the average rate for the 3 preceding years.*

[1] H. M. Johnson & P. W. Cobb. The educational value of "drivers' clinics." *Psychol. Bull.*, 1938, **35**, 758–766.

[2] H. M. Johnson. Born to crash. *Collier's*, July 25, 1936, **98**, 28, 58, 60.

[3] T. W. Forbes. Age performance relationships among accident-repeater automobile drivers. *J. consult. Psychol.*, 1938, **2**, 143–148.

[4] H. R. DeSilva & T. W. Forbes. Improving bad drivers. *Safety Engng*, 1938, **75**, No. 6, 13.

[5] H. R. DeSilva, R. G. Claflin, & W. J. Simon. Making safer bus drivers. *Transit J.*, 1938, **82**, No. 12, 450–451, 471.

TABLE I

RE-EDUCATIONAL EFFECT OF DRIVER TEST CLINICS

Comparison of Accident Record Before and After Clinic Operation
Tested and Untested Repeaters—9 Months
Follow-up—Manchester, N. H.—January, 1937

	Number of Accidents Accruing to Groups of Repeaters on a Basis of 100 Drivers per Group	
	Untested	Tested
Before Summons to Clinic (15 months)	195	221
After (9 months)	38	18
Accidents After as Per Cent of Accidents Before (compared on a 9-month basis in each case)	32.5%	13.7%
Advantage According to Re-education by Driver Test Clinic (compared to police attention, safety campaigns, etc., alone)		57.5%

TABLE II

RE-EDUCATIONAL EFFECT OF DRIVER TEST CLINICS

Drivers Involved in Accidents, Before and After Clinic Operation
Tested and Untested Repeaters—9 Months
Follow-up—Manchester, N. H.—January, 1937

	Untested	Tested
Drivers in 2 or More Accidents Before Summons to Clinic (15 months)	79	101
Drivers in 1 or More Accidents After (9 months)	21	16
Drivers in Accidents After as Per Cent of Those Before Clinic Operation (compared on a 9-month basis in each case)	45%	27%
Reduction by Driver Test Advantage According to Clinic		40%
Per Cent of Drivers Showing 2 Accidents Before and Any Accident After	48%	20%
Advantage of Clinic Re-education		48%

TABLE III

ACCIDENTS FOR BOSTON ELEVATED RAILWAY GROUP BEFORE AND AFTER CLINICAL EXAMINATION

	June 1937 June 1938	1936	1935	1934
Number of Chargeable Accidents	99	132	154	131
Number of Exonerations	140	131	115	146
Total Accidents	239	263	269	277

Chargeable Accidents

Number of Accidents per Year for the Past 3 Years	139
Number of Accidents from June 1, 1937 to June 1, 1938	99
Decrease in Number of Accidents	40
Percentage Improvement	28%

Total Accidents

Number of Accidents per Year for the Past 3 Years	269.7
Number of Accidents from June 1, 1937 to June 1, 1938	239.0
Decrease in Number of Accidents	30.7
Percentage Improvement	11.3%

A REPLY TO LANIER'S NOTE ON 'MOTOR SPEED AND TEMPO'

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In a note on the writer's [1] review of the literature on "Factors conditioning motor speed and tempo," Lanier [2] has accused the writer (1) of misrepresenting the position taken by Lambeth and Lanier [3] in their paper on "Race differences in speed of reaction," and (2) of erroneously labeling such a position as "antiquated" and "mentalistic."

In the article under consideration, the Stanford-Binet scale, a rational learning test, and 6 tests of rate of response were given to 30 White and to 30 Negro 12-year-old boys. The intercorrelations among the various tests were found to be higher in the case of the Negroes. This fact, according to Lambeth and Lanier, "seems to argue for the *relatively greater operation of a 'general factor' among the conditions which DETERMINE Negro performance*" (p. 278 of original article, quoted on p. 31 of note by Lanier; italics and caps mine). They add: "This higher intercorrelation might conceivably be due to any one of several causal factors or to a complex of factors. We have already mentioned, and dismissed as far as these data go, the possibility that greater heterogeneity in the Negro sampling might operate to produce these higher correlations. It has been suggested that the presumably more differentiated environment of the White might produce a greater specialization of 'abilities' which would result in a lower average intercorrelation among test scores for that race. *Or the Negro might be basically of a simpler, more homogeneous, more 'primitive' type of organization.* We cannot conclude from our data which hypothesis, if either, is correct, and speculation is useless. The problem must be left, as indicated above, to future research" (p. 278 of original article, quoted on pp. 31–32 of note by Lanier; italics mine).

If one were to omit all consideration of possible sampling errors, of the hazards in generalizing from such a few cases, and of the questionable use of the fictitious concept of "race," one could still question the logic involved in the second hypothesis italicized above. It will be noted that the remarks of the writer to which Lanier objects relate to this hypothesis. It will also be noted that the "fact" that "the Negro might be basically of a simpler, more homogeneous, more 'primitive' type of organization"

[1] J. P. Foley, Jr. Factors conditioning motor speed and tempo. *Psychol. Bull.*, 1937, **34**, 351–397.

[2] L. H. Lanier. A note on Foley's review of the literature on 'motor speed and tempo.' *Psychol. Bull.*, 1939, **36**, 31–32.

[3] M. Lambeth & L. H. Lanier. Race differences in speed of reaction. *J. genet. Psychol.*, 1933, **42**, 255–297.

is put forth by Lambeth and Lanier as a possible *causal factor* in the results which they obtained (*i.e.* the higher intercorrelations), not merely as a statement of the results themselves. Lanier's verbal homage to operationalism is worthy indeed, although any attempt to give an operational definition of the "causal factor" of the Negro's "simpler, more homogeneous, more 'primitive' type of organization" would indeed be a feat in symbolic logic as well as in a new and hitherto undiscovered scientific physical anthropology. Such concepts have no basis in empirical fact, are scientifically meaningless, and, I am forced to repeat, smack of mentalism and—I am optimistic enough to believe—antiquity.

To Lanier's statement that "the current concept, 'mental organization,' carries no 'mentalistic' connotations in the dualistic sense" (p. 32), I can only refer, by way of example, to Thurstone's recent critique [4] of Anastasi's monograph on the influence of specific experience upon mental organization.[5] Thurstone, an acknowledged pioneer in the field of mental organization, repeatedly alludes to the statistically derived factors as "basic human abilities" (pp. 230–231) or "mental faculties" (pp. 234–235), and regards such factors as underlying psychological entities or "faculties" which the subject *uses* when he solves a test problem or responds to any stimulus situation. Thurstone thus introduces a sharp dichotomy between *ability* and *behavior*.[6] It will be noted that this is the same type of mentalistic thinking employed by Lambeth and Lanier in their hypothesis attributing the higher Negro intercorrelations to the possible operation of "a simpler, more homogeneous, more 'primitive' type of organization." The antiquated characteristic of such doctrines should be clearly apparent to anyone familiar with the elementary principles of logic, even when these doctrines are smuggled into the framework of an otherwise empirical psychology.

[4] L. L. Thurstone. Shifty and mathematical components: a critique of Anastasi's monograph on the influence of specific experience upon mental organization. *Psychol. Bull.*, 1938, **35**, 223–236.

[5] A. Anastasi. The influence of specific experience upon mental organization. *Genet. Psychol. Monogr.*, 1936, **18**, No. 4, 245–356.

[6] Cf. A. Anastasi. Faculties *versus* factors: a reply to Professor Thurstone. *Psychol. Bull.*, 1938, **35**, 391–395.

BOOK REVIEWS

MURRAY, H. A. Explorations in personality. New York: Oxford Univ. Press, 1938. Pp. xiv+761.

This volume, which was written for the most part by Professor Murray, represents "A Clinical and Experimental Study of Fifty Men of College Age by the Workers at the Harvard Psychological Clinic." The first chapter introduces the problems which are considered and, in general, the viewpoint which guided the studies.

The second chapter, "Proposals for a Theory of Personality," is a very long and involved argument advanced by Professor Murray to clarify the philosophical considerations surrounding the particular viewpoint which he has adopted. He considers in detail the advantages accruing from the study of "internal" man rather than from the study of "external" man. He considers in all its aspects the problem which others have dealt with under the concept of motivation. This, in Murray's terms, is really the problem of *need*. His concept of need is carefully differentiated from the concepts of drive, traits, attitudes, motivation, etc., so that, philosophically, his approach has all the limits stated and defined. The notion of the importance of the genetic development of the internal life of the individual in relation to the adult personality is carefully formulated. The line of argument depends on evidence and hypotheses previously developed by the psychoanalysts, but broadens them and fits them into a wider and more inclusive scheme of thought. This chapter is long and covers a wide range of psychological points. The theories and concepts are sometimes clearly expressed, but at other times the final product does not seem to justify the long arguments which have been used to maintain or elaborate the point. As a general psychological orientation it is at the same time illuminating and baffling.

The third chapter, "Variables of Personality," presents the anatomical structure which Murray proposes for use in the descriptive analysis of personality. It is extremely difficult reading and probably can be justified only in terms of the outcome. If the particular system of description and definition will lead us either now or later to a more complete and better understanding of personality, then the effort has been justified; otherwise, the conceptual framework is merely a logical play on words which seems intuitively correct but has no counterpart in reality. Whether this anatomical structure turns out to be a psychological, or merely a logical, construct remains for usage to determine.

Chapter IV, "Judgments of Personality," has been written by R. Wolf and H. A. Murray. Interesting material is presented here, bearing on the way in which opinions may be synthetically obtained from putting together both experimental findings and judgments by qualified experts. The authors point out that the data are insufficient and the sources of

error many, but it seems that they have approached a problem which is central and important for all psychological work of this variety.

Chapter V, "The Genetical Investigation of Personality: Childhood Events," is an excellent statement of the factors out of infancy and childhood which are of real importance in the adult personality. Again, most of this material has been brought out and elaborated by psychoanalysts. Murray's use of this material is carefully thought through, and the orientation of it to the general field of psychology is well defined. It seems to the reviewer that a great deal of valuable thought and material is presented here which might well be incorporated into general academic textbooks.

Chapter VI, "Procedures," describes some twenty-five or thirty procedures and experiments administered by the various workers at the Clinic to the individuals studied. Some of these experiments are given in detail, with their proper psychological setting and their relation to the entire problem of personality pointed out; others are sketched in a brief and unsatisfactory fashion.

Chapter VII, "The Case of Earnst," is a long and detailed case history which presents the direct application of the philosophic viewpoint and experimental procedures which have been outlined in the previous chapters.

In Chapter VIII, "Conclusions," Murray summarizes his viewpoint concerning the value of the work reported.

First, the reviewer will present the points which he considers to be positive features of this study. The project in its entirety is psychology in the strict sense of that term. It is dealing with that aspect of human existence which everyone, except certain professional psychologists, believes to constitute the field of psychology. It deals with the inner life of the individual and the ways in which this inner life is modified by, or modifies, the environment. A working terminology is developed, and a real attempt has been made to use the terminology consistently. All too frequently such terminologies have been presented in the past and then never brought into practical application.

This is a frank, open, dynamic approach which draws much of its orientation from Freudian psychoanalysis. However, it is bigger, more inclusive, and more sophisticated than anything so far presented by the Freudians. It lacks the mysticism and poetry so inherent in previous psychoanalytic presentations. Intuition is frankly acknowledged, accepted, and given its rightful place in a scientific procedure without recourse to unnecessary spirituality.

The explorations are ingenious; most of the methods are shrewd and intelligent; the application of the method of medical consultation for diagnosis and prognosis to the problems of psychological study of personality is, generally speaking, new in psychology, and the transfer from medicine to psychology has been well done. The author has attempted to predict wherever possible. He has not hesitated to use good judgment and such knowledge as he or his colleagues possessed in stating the possible outcome of factors which they have determined.

We have had endless discussions in psychology concerning the study of the personality as a whole. Mostly, these have been verbal fireworks which were not backed up by anything more than a phrase and a desire. Murray and his colleagues have actually attempted to do what others have spoken of for years. They have orientated their problem with reference to all present psychological knowledge and have not handicapped themselves by adhering blindly to any particular preconception, such as some specialized brand of psychoanalysis, trait psychology, etc. The broadness of the approach and the attempt at the same time to keep their lines of communication open are most commendable and have produced excellent results.

The negative points which have impressed the reviewer are next in order. Personally, I dislike the terminology and the way in which the terminology all too frequently obscured the meaning which the authors were trying to present. I know that psychology stands in great need of a working vocabulary which can be used in the field of personality. It seems to me that the vocabulary which the Harvard Clinic has presented is at present awkward, at times inexact, and in part meaningless. Whether future work will make this terminology a smooth-fitting garment suitable to clothe adequately the presentation of the facts of the study of personality remains to be seen.

This volume contains an overload of philosophy and theory. The extended discourse of Chapter II seems unnecessary. Pragmatically speaking, the study should demonstrate its own worth without the long verbal defense. A statement of the viewpoint and the assumptions underlying the work is all to the good, but the compulsion to relate this viewpoint to all other varieties of knowledge seems at present a wasted effort. It is exceedingly difficult and at times impossible for the reader to distinguish between experimentally determined fact and preconceived or derived theory.

The two most glaring deficiencies in the work are in the selection of experimental methods and the statistical expression of the results. Many of the experiments are ingenious. They are new and carefully thought out. However, other experiments which are used are unbelievably naïve. When one considers the vast amount of work which has been done on learning or on the psychogalvanic reflex and then views the experiments which were supposed to bear on these two measurable aspects of personality, one feels that the work which academic experimentalists have been doing for the past eighty years has been done in vain. I suppose that this is due to the fact that certain of the workers at the Harvard Clinic were students beginning graduate work who had no concept of the vast field of experimental psychology which is available in the literature. Certainly, several of the experiments reported give no reference to the body of previously determined experimental facts which should have added a great deal of meaning to the material.

The presentation of statistical evidence is variable. Certain of the workers present tabular statements of correlation, probable error, etc.; other investigators report their results in such a fashion that only the

most careful examination of the entire report gives the reader any notion concerning the true validity of the results. For example, the statement may be made that a correlation of +.40, obtained in a certain experiment, is significant. Careful reading will show that the +.40 may be based on but fifteen cases, with no statement of scatter, probable error, steps, etc. Of course, the viewpoint and the experimental procedure did not demand particularly careful or elaborate statistical presentation. However, I do believe that, in general, a more adequate mathematical treatment of the results might have altered the conclusions and probably would have brought about more positive suggestions for further work.

A diagnostic council of experts was continually informed of the outcome of each experiment in order that they might prognosticate the outcome of future experiments as well as suggest new experiments. Their work constituted the keystone of the entire study. Unfortunately, the work of this committee is not adequately reported. There is no basis for judgment of how well or how poorly this council functioned. The statement is made that the council became more accurate and adequate with practice. However, one cannot learn whether, at the end of the experimental program, they had a basis of prediction which was 90% correct or 50% correct.

The case history which is given as the proof of the method leaves much to be desired. It is a nice anatomical dissection of a personality, but it certainly is weak in functional prediction, and functional prediction was the end-product desired. Furthermore, the various experimenters who report their findings do so in differing terms. Having read through the complete case, one is impressed by the "sour" or ultrasophisticated attitude of many of the experimenters. Actually, Earnst impresses the reviewer as a commendable person with outstanding personality assets, yet if many of the workers at the Clinic did have any appreciation or understanding of these assets of Earnst, they certainly carefully concealed such knowledge and failed to report it.

The dust cover of this volume contains the phrase "Let Not Him Who Seeks Cease Until He Finds, and When He Finds He Shall Be Astonished." This really characterizes the study. It has been a gallant attempt; it is overloaded with philosophy, and suffers from poor experimental work in a fair number of experiments. Certain of the workers at the Clinic present brilliant and outstanding experimental findings and formulations which save the project as a whole. This study might be compared to an attempt to reach the top of Mount Everest. Other psychologists may learn by the failures of this expedition as well as other expeditions which have gone before. Those who took part in the expedition know better than anyone else who contributed and who failed. But over and above everything else the project again demonstrates the value of trained investigators who can make use, in an intelligent and ingenious fashion, of all previously accumulated psychological knowledge from whatever source.

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KÖHLER, W. The place of value in a world of facts. New York: Liveright, 1938. Pp. ix+418.

Köhler opens his book with the clever literary device of discussing with an editor friend "the case against science." The discussion is precipitated by the realization that science faces a crisis of its own making. Science has lost its esteem and has generated disaffection because its devotees, having willfully neglected the needs of everyday man, have retired to their ivory tower where they carry on their own technical pursuits. Science does not deal with values, but only with facts. According to the indictment, this is no less true of the social sciences and psychology than of physics. The remaining nine chapters are devoted to the demonstration that there is a place—and a large one—for values in a world of facts.

The *mise en scène* of Köhler's trial of science is Germany, though the conditions there are typical of all places. According to the critic, science risks the result not only of its professors being replaced by other teachers who would be followed by the people, but science itself, feeding upon negativism, will perish of its own inanition.

Though this literary device excellently serves Köhler's purpose of stating a problem designed for his own type of solution, the critical reader might be spurred on to ask the following questions: Is it true that all scientists are negativists? Do all scientists exclude sounds, sights, and other concrete qualities from the world of nature, on the plea that these are nothing but human phenomena? Does science, or do only some scientists, really divorce values from facts? Are facts merely abstract quantitative determinations and therefore *ipso facto* exclusive of the qualitative characteristics of events? Is science obliged to feed and clothe the world in addition to its help in providing the means for supplying such needs? Is science, without politics, economics, law, and custom, to be the scapegoat upon which to heap the sins of men, even of those who suffer from their neglect to learn their responsibilities as wise citizens who vote properly and deal justly with one another? These questions notwithstanding, the reader must agree with Köhler that the articulation of values with other phenomena is as interesting as it is important. In consequence, he will follow Köhler's treatise with deep absorption.

In order to establish his contention that values and facts are not sharply set over against each other, but rather that values are essential features of nature, the author critically examines (Chap. II) a number of prominent current value theories. The study of these theories results in the conclusion that value means, essentially, requiredness. What requiredness is he proceeds to show (Chap. III) by demonstrating that "certain facts do not only happen or exist, but issuing as vectors in parts of contexts, extend toward others with a quality of acceptance or rejection" (p. 100). In other words, they belong together. "Thus value-situations fall under the category of gestalt" (p. 86). This result is achieved by the phenomenological method. When employing intuition, introspection, and what philosophers call immediate experience, Köhler discovers that "fact" is an ambiguous term. "Not all facts are 'indifferent facts'." "Within certain factual contexts the requiredness or

wrongness of some facts is no less real than is the existence of these facts" (p. 102).

Naturally, Köhler is not content with merely establishing values in the precarious phenomenological realm. Though "phenomenology is the field in which all concepts find their final justification" (p. 102), he cannot forego reaching out toward another and more scientific realm—the physical world. And so in Chapter IV, entitled "Beyond Phenomenology," he attempts to show how it is possible to transcend this realm. The task is accomplished by memory and its underlying neural traces. Since memory involves, essentially, a reference beyond itself, Köhler believes that not only is transcendence beyond the phenomenological world established, but also at the same time transphenomenal existence is implied (p. 118).

Not that difficulties are wanting here, for the author regards the physical world as of such a character that it is not directly accessible to man, but inferentially derived from the observation of percepts (p. 142). So great is the difference between the two that the physical world exists only through a procedure of construction (*ibid.*). Chapter V, however, is devoted to showing that there is an invariable relationship, an isomorphism, between the perceptual or phenomenological and the physical worlds. As is quite to be expected, isomorphism looms large in Köhler's thinking as a necessary and sufficient factor. It is necessary, since he rejects, for example, Eddington's idea that all observation in physics is reduced to pointer readings or that pointer readings are the only perceptual materials employed by the physicist in his research and scientific construction, thus leaving ample place for phenomenal or mental components; and it is sufficient because, by emphasizing this relationship, he deems himself capable of crossing from one to the other. Because he cannot entertain the possibility that science can dispense with such a dualism, we read between his lines the analogue to Archimedes' cry: Give me gestalt and isomorphism and I will unite the absolutely incommensurable psychical and physical.

As in his other writings, so in this volume, Köhler makes use of the brain to bring together the phenomenal or mental and the physical. Following traditional dualism he relies upon the nervous system to make requiredness or value a factual as well as a phenomenal or mental affair. In Chapter VI the author elaborately expands his doctrine of psychophysical, really psychoneural or psychocortical, isomorphism. Characteristically, the fundamental point is, of course, that there is a resemblance between experience itself and the cortical correlates of experience. "The fact which mediates between the physical and the perceptual structure is now found to be *cortical* organization, which, as a rule, resembles both" (p. 218).

In Chapter VII, entitled "On Memory and on Transcendence," the author applies his psychocortical theory in the form of traces to establish requiredness and the coincidence between facts and values. He argues that not only is there transcendence in simple, successive comparison as well as in long-range memory, but there is requiredness also, as when, for example, we try to recall a name and realize that a standard beyond

informs us that this name is wrong and that one is closer, etc. (p. 276). "Once more we have an amphibian context; requiredness is its most striking trait; and the vector, which in this context objects to one thing or readily accepts another, owes its character as a quite specific demand entirely to the properties of a trace" (*ibid.*).

After a rather thorough examination of the general organic field (Chap. VIII), Köhler concludes that "no procedure of science reveals any actual participation of demands and values in the determination of organic events" (pp. 327 f.). But despite the fact that we do not find requiredness as such among the data of science, a general trend of nature may yield the same results as if events occurred in order to fulfill a demand.

In the field of forces, however, Köhler finds great similarity to requiredness (Chap. IX), since forces constitute factors in dynamic contexts or situations in which they transcend from certain parts to others and point beyond present existence to future changes. Accordingly, the author is satisfied that nature comprises a dualism of facts and oughts.

In his closing chapter Köhler considers the general question of the extrusion of man from physics. The physicist has extruded him in order to avoid adding subjective ingredients to his system, while students of human phenomena have done so because they could not abide the idea of physical phenomena influencing the mental. To harbor both in one system is to Köhler not only proper but essential for scientific purposes. His own view is that physics itself is the work of man and it is through him that inevitable connection exists between the phenomenal and natural worlds. Man as a link between the two is, for example, illustrated by the fact that human motives are "identified with physical tensions which develop between the cortical correlate of the self and that of an object" (p. 377).

In appraising this volume we cannot laud too highly the investigation by a scientist of such important phenomena as values. It is a distinctive merit to divest oneself of the conventional phobia for everything which is not clearly a set of far-fetched abstractions. Too often scientists confuse the fact that science must attain to abstractions with the idea that they must exclusively occupy themselves with abstractions. But still more praiseworthy is Köhler's attitude of studying values on a scientific level. In so far as he carries out this intention he avoids the paradox of recognizing the importance of values and then interpreting them as sentimental and mystical entities really unrelated to natural phenomena.

Unfortunately, the author heavily weights the debit side of the balance. His actual performance does not square well with his intentions. Though he aims to accord values a definite place in nature, he, after all, treats them as tenuous abstractions because for him nature itself turns out to be a set of abstractions. In his final analysis nature becomes reduced to the constructions of physics, while the objects and processes with which the scientist begins and which set him his problems are converted into phenomena in the sense of subjective states and qualities. In consequence, Köhler finds himself dealing with different kinds of worlds. True enough, he wants to keep the number of such worlds down to two, that of physics

and that of subjective states, and for this purpose adopts Occam's razor in the form that "worlds ought not to be multiplied beyond strict necessity" (p. 236). He does not abide by the rule, however, in view of the fact that nature is, and science knows, only one world. All the rest is phantastic and unnecessary construction.

Basic to Köhler's unacceptable treatment of both facts and values is his inadequate conception of scientific construction, which he limits to the work of the physicist. That the subjective or introspective states or qualities are likewise constructions he does not appear to consider. In fact, Köhler definitely accepts such inadequate and invalid constructions as psychic states and assumes that they are crude data or the given of science. It may be suggested that the crude data of science are always the concrete phenomena of everyday life which initiate all scientific activity. Scientific work begins by an actual interbehavior with such phenomena, and its results may be summed up as: (1) the construction of descriptions and explanations on the basis of such interbehavior; (2) the discovery, by means of such contacts with the original data, of other, at first unsuspected, crude data; and (3) the testing of such constructions by ascertaining whether they comport with the phenomena of the original contacts.

It is only Köhler's unsatisfactory conception of science and its work that prevents him from treating values as definite phenomena in human situations. When we assume that science operates in a domain of contacts with concrete phenomena, the scientist has a larger range of activities than that circumscribed by conceptual physics. In this larger domain the student of values does not face the problem of how values can be assimilated to a world of physical abstractions. Consequently, no heroic measures need be adopted to prove that values are factors of a cosmic dynamics or that the dynamic world harbors objective strivings. In other words, Köhler's quixotic measures are supererogatory devices made necessary by accepting a traditional dualism such as the scientists of the Seventeenth Century established.

Not only does Köhler's dualism interfere with an adequate treatment of values; it likewise results in a flagrant misinterpretation of facts. For example, when Köhler attempts to establish his resemblance between the phenomenal and the physical, he does so by means of the brain. It is a serious predicament to require such functions of the brain as to misconstrue completely its nature and operation. Is the brain anything other than a definite biological mechanism for the coördination and integration of the activities of the organism? To make it the mediator between physical and psychic states is to go completely counter to the teachings of biological science.

A curious paradox issues from Köhler's use of the brain as a metaphysical device; namely, a thorough contradiction of what the reviewer, among others, regards as the primary contribution of Gestalt psychology. It is no small addition to psychological theory to interpret a psychological phenomenon as a concrete interactional relationship between an organism on the one hand and an object on the other. For example, when two unequal parallel lines are presented, the subject responds to

them as though they were parts of a trapezoid or a rectangle, as a function of the distance between them. Now, instead of dealing with these situations as two objective relations between activities of individuals and line configurations, Köhler prefers to use the brain as a mediator between phenomenal or psychic geometry and physical gradients. Resort to the brain hypothesis reduces psychological description entirely to internal principles in the organism.

Does not Köhler thus employ the culturally established dualistic tradition to misinterpret experiments in order to establish values among facts? In view of neurophysiological findings it turns out that interpreting experiments cortically not only does not establish values, but rather casts suspicion upon Köhler's notion of values. How can such values aid us in dealing with the insistent problems of human affairs?

And finally, it is significant to what extremes the author resorts in his argument for dualism and its cortical intermediary. First, he regards the objection to the brain explanation as emotional bias on the part of the objectors. He counsels courage to adopt a dualistic instead of a materialistic-monistic metaphysics without considering the advisability of rejecting traditional metaphysics altogether. His own courage is morally supported by calling neural action psychophysical and distinguishing between the "body" as percept and the organism as a transphenomenal entity (p. 187). In the second place, he employs the hypothesis argument: In order to account for certain phenomena it is necessary to introduce the brain hypothesis even though "no such traces of neural events can yet be demonstrated by neurological methods" (p. 237). In support of his view he suggests that the atomic theory was rejected in the Eighteenth and Nineteenth Centuries for lack of evidence and that when direct evidence is available it is no longer an hypothesis. The obvious reply, of course, is that scientists make mistakes and that to accept any proposition as an hypothesis is to make way for every kind of occultism on the ground that some day it may be established. This was not true, by the way, for the caloric, phlogiston, microzyme, and gemmule hypotheses, among thousands of others. Moreover, in the case of the neural hypothesis we know its origin in historical tradition, and certainly the accumulated knowledge of neurology increasingly shows the exclusively biological and nonpsychical character of the brain.

It is a source of regret to the reviewer that he finds it necessary so adversely to criticize such an admirably motivated and learned a book. In extenuation it may be said that it is precisely the necessity to establish values in a world of facts that requires us to obtain a proper orientation toward this most important and elusive problem.

J. R. KANTOR.

Indiana University.

BECK, S. J. Personality structure in schizophrenia: a Rorschach investigation in 81 patients and 64 controls. New York: Nervous & Mental Disease Publishing Co., 1938. Pp. ix+88.

This monograph attacks the problem of the Rorschach test in schizophrenia from various sides. For purposes of demonstration, a representa-

tive record each of a normal and of a schizophrenic person is given in full with their scores, the latter being subjected to a detailed analysis and psychological interpretation. Except for this example, the material is dealt with in a statistical way, *i.e.* the numerical data for the various test categories in the schizophrenic group are scrutinized and paralleled with corresponding data from the nonpsychotic group. Finally, the psychological implications of the findings are discussed at some length.

Everyone who has had some experience with schizophrenic "Rorschachs" is acquainted with the wide variety of reaction forms that one finds among these patients. It is therefore rather unfortunate that only one such record is reproduced in its entirety, especially in a monograph devoted to the subject. The presented case is well chosen, however. It brings out as many features characteristic of the disease group as can be expected in a single record, particularly since it is compared with the responses of a control subject of the same sex, educational level, and of similar age. To beginners in the field, this case analysis may be warmly recommended. The analysis begins with a careful consideration of the psychological significance of the numerical values of each test category. A very valuable discussion of the relationships among these categories follows. The reader is warned against interpretations made on the basis of single test findings taken out of the context of the whole record. By means of a diagram the interdependence, or inner balance, of the major categories is well illustrated. One regrets, however, that the analysis of the individual record has not been carried further into the finer, more qualitative aspects of the test, such as one finds in the well-known *Rorschach-Oberholzer* study or in some publications of the *Rorschach Research Exchange*.

The statistical section is the best. Means, medians, Q's, S.D.'s, ogives, and critical ratios are utilized to arrive at the following list of test categories, as differentiating the two groups in a statistically significant way: Dr, DW, C, CF, FC, F+, and P. M, in contrast to Rorschach's contention, is not higher in schizophrenics than in normals. Unfortunately, original responses, which have proven a useful differentiating criterion, are not considered by the author. On the whole, however, this examination of the various test categories lends gratifying corroboration to Rorschach's own, Skalweit's, and the reviewer's findings in schizophrenic patients. Important, too, is the stress on the great variability within the schizophrenic group, well demonstrated by a histogram. Particularly illustrative is the diagram representing the relationship of color to movement.

The main ideas of the section dealing with the structure in personality are familiar to the reader of the author's earlier publications, especially of his book, *Introduction to the Rorschach method: a manual of personality study* (1937). The whole personality is thought of as consisting of a psychological and of a social personality. The former refers to "certain major psychological processes . . . certain traits adding to finer lineament . . ."; the latter is "the coloring which the individual has taken on by reason of his personal experience . . . the ideas and special reference to his wishes . . ." Differences in personality structure are con-

ceived chiefly in terms of balance between the major psychological processes, as yielded by the Rorschach test. Thus, in a superior normal the main personality factors are all present in high quantities. In a person of lower, but healthy, endowment all the processes are reduced proportionately, still preserving the balance. The abnormal person, on the other hand, is characterized by some imbalance in his structure. The discussion culminates in some rather vague speculations as to whether personality patterns are limited in number and how accurate personality measurement can be. After this general treatment the statistical findings on the schizophrenic group are taken up again in order to construct a sketch of the schizophrenic personality in terms of the above principles.

Besides the parts of the book discussed here a description of the Rorschach method and a bibliographical note are included.

In summary, one might say that this should be a useful book. One wishes, however, that the author had used some of his broad experience in bringing out more fully the significance of the total individual record in all its quantitative and qualitative potentialities.

MARIA RICKERS-OVSIANKINA.

Wheaton College.

VAN DER HOOP, J. H. Bewusstseinstypen und ihre Beziehung zur Psychopathologie. Bern: Hans Huber, 1937. Pp. 375.

In this book Van der Hoop continues his work of modifying and applying Jung's typological system. The first principle of classification is still that of extraversion-introversion. Psychiatric experience and the use of Husserl's phenomenological procedure have led Van der Hoop to make changes in the four mental functions which provide the second principle of classification. Jung's sensation type has been replaced by the instinctive type, "instinctive" being used almost in its popular sense: natural-feeling, unpremeditated, impulsive. The instinctive function includes direct emotional reactions and direct impulses to overt behavior; as described in detail, it is calculated to fit a large number of people who are so natural and sometimes so ordinary that they are likely to elude a typological classification. The feeling function has been broadened to include sentiment (the concept offered by McDougall and Shand), as well as affectivity and mood. The functions of intuition and thought, and the corresponding types, are more nearly the same as Jung's. There is presumably a native disposition which determines the dominance of a particular function and the appearance of the resulting personality type. However, the classification of a given person may be complicated in two ways (among others): by the presence of a second one of the four functions in lesser degree; and by "polar" action, the contradictory action of a function which is incompatible with the principal function.

Van der Hoop uses explanation in terms of personality type to supplement, rather than to replace, explanation in terms of temperament and the Freudian mechanisms. If a given person is to develop a neurosis, his personality type may determine what form the neurosis will take—Freud's problem of the *Neurosenwahl*. The typology is especially useful

in understanding constitutional psychopathic personalities, which appear as caricatures of the normal personality types. There are also interesting applications of typology to the problems of mutual understanding and the psychology of psychologists. Topics in epistemology, developmental theory, and the psychology of religion are handled with distinctly less success.

It is encouraging to find an honest and serious typological work, especially in view of the incredible perversions of typology, inspired by racial prejudice, which have appeared elsewhere. Some of Van der Hoop's type sketches are so convincing that they suggest certain actual contemporary people, and include the most puzzling features of these people. It might very well be that by means both subtle and rigorous one could confirm some of the assertions of this typology. But its systematic concepts are taken chiefly from a moribund-act psychology and are hardly quickened into life by the use of phenomenology. The basic difficulty is one of nonoperational definition. We know very little of the operations which lead to introspective observation of the larger features of experience, and so cannot take advantage of phenomenology or its statements. The direct "intuitive" judgment of other people is equally resistant to operational analysis and equally unsatisfactory as an unaided source of scientific psychology, whatever may be its importance in everyday life. If these operational difficulties are widely appreciated, a typology founded on phenomenology and personal observation will have hard sledding.

JOHN VOLKMANN.

Columbia University.

VILLEY, G. La psychiatrie et les sciences de l'homme: essai de synthèse scientifique. Paris: Félix Alcan, 1938. Pp. 194.

This essay, which is addressed to both the profession and the educated public, has three aims. It attempts to introduce psychiatry as a science, to show how psychiatry is related to medicine and the other sciences, and to establish the author's views on nosology and etiology. This is a big order for less than 200 pages and the result is a very uneven hodgepodge. The educated layman will gain nothing from the technically difficult and methodologically confused sections on psychological language and nosology; the professional psychiatrist will be bored stiff by the sketchy discussions of history, of the scope of psychiatry and its relations to the other sciences, and of hospital care and management. It is unfortunate that the author chose to combine his original theses in a book which otherwise might be considered an eloquent and modern plea for psychiatry to the educated Frenchman.

The author's personal psychiatric contributions are not worth much consideration. He follows the ideas of Chaslin on psychological language and of Janet on nervous equilibrium and attempts a synthesis of these viewpoints. Both of these ideas are good as far as they go, but they never have gone very far, and the reviewer is unable to see that the author has advanced with them. There is no indication that Villey is

familiar with either modern American or German psychiatric theory. A few references to Freud indicate his totally inadequate conception of psychoanalysis. The sociological theses occasionally advanced are hopelessly outdated and naïve. There are quite a few moral and ethical evaluations which do not belong to modern psychiatry. The book contains a short and very weighted bibliography but no index.

J. F. BROWN.

University of Kansas.

ROBERTS, H. The troubled mind: a general account of the human mind, and its disorders and their remedies. New York: Dutton, 1939. Pp. iv+284.

FLETCHER, P. Life without fear. New York: Dutton, 1939. Pp. 111.

These two books are, from internal evidence, of British origin, reprinted for popular sale in this country. Dr. Roberts' book, with several chapters on the insanities by Dr. Margaret N. Jackson, is an entertainingly written, middle-of-the-way account of human psychology, especially with respect to the problems of adjustment. The author's psychology leans rather heavily upon innate traits and on a limited psychoanalysis. While of little interest to the professional psychologist or psychiatrist, it will probably be appreciated by intelligent laymen. Dr. Jackson's chapters include very brief, conventional descriptions of the common types of mental disorders.

Fletcher's little book emphasizes fear as the basis of maladjustment, and the point of view is sufficiently given in one quotation: ". . . I have reached the conclusion that no method of therapy will effect permanent and constructive changes in human behaviour except to the extent that it is based upon a realization of the spiritual nature of man, and reverence for personality as the instrument of God for the expression of His nature and the fulfillment of His will."

C. M. LOUTTIT.

Indiana University.

WALTON, R. P. Marihuana: America's new drug problem. Philadelphia: Lippincott, 1938. Pp. ix+223.

The book summarizes what is known of the use of this drug and its kindred forms, such as hashish, all derived from the hemp plant. Outlined in separate chapters are the history of the hashish vice—which is very ancient, its present distribution throughout the world, and the present status of the marihuana vice in the United States, this latter contributed by Frank R. Gomila and Madeline C. Gomila of New Orleans. The use of this drug in some form is very widespread throughout the world and has increased rapidly in the United States and Canada during the last few years.

One difficulty with the suppression of the vice is the ease with which the drug may be procured. The hemp plant is easily obtained inasmuch as its fibers are of such great economic value. The plant adapts itself easily

to almost any habitat and can be successfully grown almost anywhere in the civilized world and in favorable habitats actually survives as a weed. Also, it is easily administered by ingestion or by smoking, and it is not necessary to prepare or purify those parts of the plant which are used.

Three chapters are given to descriptions of the hashish experience from the subjective, objective, and psychiatric points of approach. Subjectively, the subject experiences a euphoria and either a complete anesthesia or at least a hypoesthesia. Many of the effects are similar to those described for peyote (mescaline). Some space is given to the chronic effects of the drug as well as the acute effects. The remaining chapters treat therapeutic applications, pharmaceutical and chemical considerations both of the drug and of the active principle and nomenclature. There is a bibliography of 419 titles.

The book cannot fail to be of interest to psychologists working in many different fields. It must be of interest to the psychologist who is interested in abnormal psychology and the psychology of drugs and also to those interested in psychiatric problems. It must also be of great interest to the psychologist who is interested in social psychology because it presents a problem of social change of recent date in the United States—so that the beginnings may still be available—and a social change which the authors show to be of very considerable magnitude. In this regard, the authors point out that many tons of hemp have been grown for fiber for a great many years in the United States without any considerable report of drug addiction. Hence, the availability of information and of the drug itself are not the only factors in its use in proportions such as those existing at present. The authors point out that at least two other factors must be present. The first is a "reasonably intimate social contact between an uninitiated population and a population which practices the vice; the other is the temperament and social conditions of the people who adopt the practice."

SAMUEL W. FERNBERGER.

University of Pennsylvania.

FRITZ, M. F. Collection and presentation of statistical data in psychology and education. New York: Prentice-Hall, 1939. Pp. vi+58.

The purpose of this booklet is clearly stated in the opening paragraph of the Preface:

"This manual attempts to present in a very simple fashion for the beginner some of the procedures in the collection, tabulation, and graphic presentation of statistical materials. It will not, of course, replace the regular textbooks on statistics or tests and measurements, but is intended to serve as a supplement. Its use will enable the student to answer certain questions for himself, thus conserving class time for other matters. Although many of the things considered may seem trivial to the trained person, the beginner finds these items a source of considerable perplexity. Therefore, no apology will be offered for the simplicity of the presentation."

This aim is fulfilled in the pages which follow; no statistical formulae are to be found, and the language and illustrations are simple and to the

point. In this connection it might be noted that a part of the material was originally written as an aid in the training of workers for a state re-employment service.

The manual is divided into forty-nine sections which include the following topics: "1. Why you should be interested in statistics"; "7. How accurate do we need to be?"; "9. Use of commas"; "12. Types of error in collecting data"; "23. Use of questionnaires"; "35. How to make graphic illustrations"; "37. Smoothing curves"; "40. The use of percentages"; "43. Label everything"; "44. Check all calculations."

A list of twenty-five discussion questions is provided to clarify further the points brought out in the text. Twenty-four books on statistics "which emphasize practical applications rather than theory" (p. 53) and an index are appended.

The pages of this booklet contain an admirable simplification of subject matter. Teachers of courses in which an introduction to the collection and presentation of educational and psychological data is a part of the work should find that this manual will fill a real need.

R. S. SACKETT.

The American University.

BOOKS RECEIVED

FREEMAN, E. Principles of general psychology. New York: Holt, 1939. Pp. xiv+530.

GESELL, A., AMATRUDA, C. S., CASTNER, B. M., & THOMPSON, H. Biographies of child development: the mental growth careers of eighty-four infants and children. New York: Hoeber, 1939. Pp. xvii+328.

GRIFFITH, C. R. Psychology applied to teaching and learning. New York: Farrar & Rinehart, 1939. Pp. xiii+650.

HOOPES, G. G. Out of the running. Springfield, Ill.: Thomas, 1939. Pp. xvii+158.

LATOUR, M. Premiers principes d'une théorie générale des émotions. (Nouvelle édition revue et augmentée: Observations complémentaires. Treizième série.) Interprétation des faits d'aliénation selon la théorie générale. Paris: Félix Alcan, 108, Boulevard Saint-Germain, VI^{e}, 1938. Pp. 55.

LINK, H. C. The rediscovery of man. New York: Macmillan, 1938. Pp. xi+257.

MOORE, H. Psychology for business and industry. New York: McGraw-Hill, 1939. Pp. xi+527.

SKODAK, M. Children in foster homes: a study in mental development. *Univ. Ia Stud. Child Welf.*, Vol. XVI, No. 1. Iowa City: University, 1939. Pp. 156.

NOTES AND NEWS

DR. WALTER DILL SCOTT, since 1920 president of Northwestern University, has presented his resignation to the trustees. He expressed the hope that his successor might be elected so as to permit his retirement in the autumn. Before becoming president, Dr. Scott had been connected with the university since 1900, successively as instructor, assistant professor and professor of psychology and director of the psychological laboratory.—*Science.*

ERIK HOMBURGER ERIKSON, formerly assistant professor in the Yale Medical School, has accepted an appointment as research associate in the Institute of Child Welfare, University of California.

A NEW Bureau of Instructional Research has recently been created at the University of Nebraska with J. P. Guilford, professor of psychology, as director. Professor Guilford will continue to devote part of his time to teaching. Mr. H. M. Cox of the University of Georgia has been appointed to full-time work in the Bureau.

THE Midwestern Psychological Association will hold its annual meeting at the University of Nebraska, Lincoln, Nebraska, on May 5 and 6, 1939, under the presidency of Dr. E. S. Conklin of Indiana University. The title of Dr. Conklin's presidential address will be "The Status of Academic Psychology." A special feature of the meeting will be the commemoration of the fiftieth anniversary of the founding of the psychological laboratory at the University of Nebraska—one of America's earliest—by Harry Kirke Wolfe.

THE Washington-Baltimore branch of the American Psychological Association held its second meeting of the year at the Waverly Press in Baltimore on January 4. The following program was presented:

G. WILSON SHAFFER: "Suggestibility in Relation to Diagnostic Types."

H. C. SMITH: "Variations in Color-Matching Ability."

VERNON SCHEIDT: "Some Industrial Applications of Psychology."

AT the recent Lima Conference a resolution was passed on behalf of the Eighth Biennial Congress of the World Federation of Education Associations to be held at Rio de Janeiro August 6–11, 1939. In essence, the resolution was to bring this Congress to the attention of educational

authorities and teachers in the American States and direct the attention of delegates to the opportunity of visiting educational institutions in other countries en route. Recognition of "the important rôle played by teachers and supervisors in the development in youth of appreciation of international understanding and good will" and the unusual opportunity afforded by this Congress to gain such knowledge and understanding prompted the resolution.

The "Rotterdam" is to sail from New York on July 5 and from New Orleans on July 10 and will provide facilities for those attending the Congress. Further particulars may be obtained by writing to headquarters of the Association at 1201 Sixteenth Street, N.W., Washington, D. C.